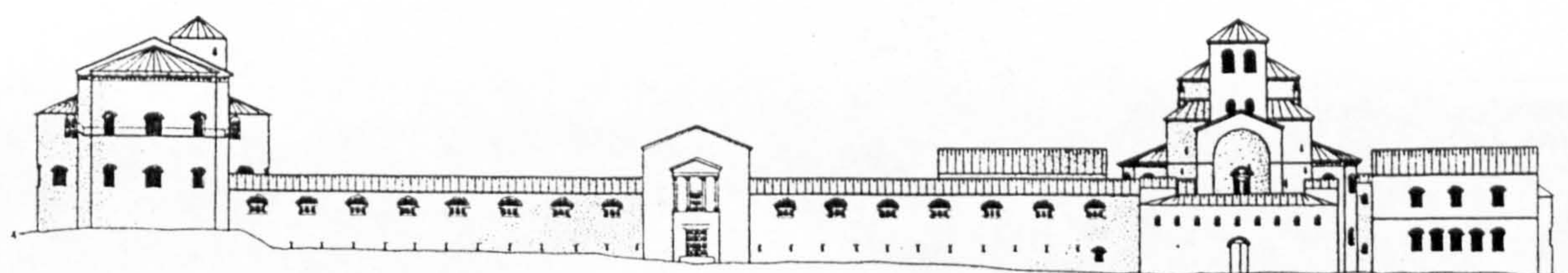
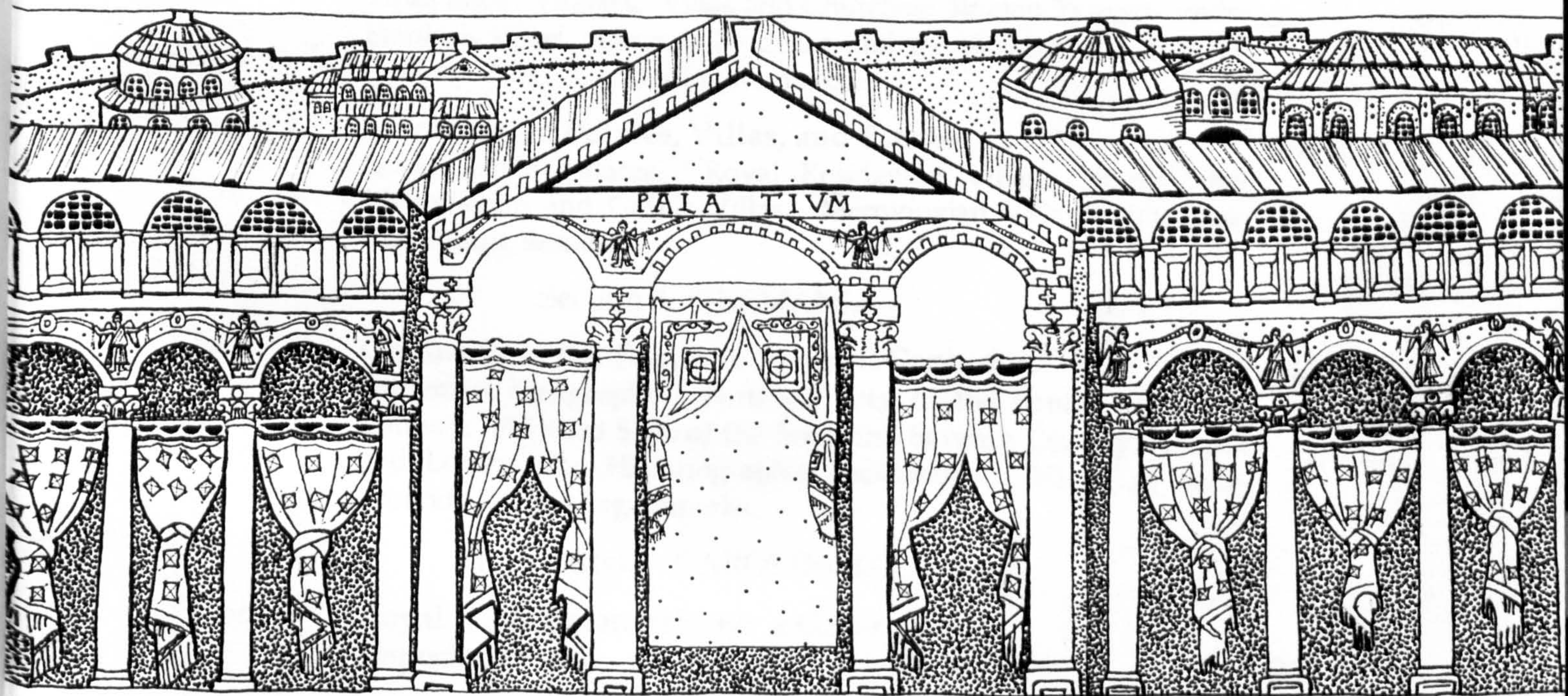


The Residences of *Potentiores* in Gaul and Germania
in the Fifth to Mid-Ninth Centuries



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BY
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Department of Archaeology, University of Glasgow.

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*To Mum, Dad, and Scott,
better late than never.*

Although churches remain to us from the Middle Ages, we have not one private house: yet nothing is more full of meaning than an old house.

- Andreas Carandini, 'A Policy for Archaeology'
Times Literary Supplement 4524, 15-21 Dec. 1989.

Chapter One

Introduction

In one respect this thesis is upside down. Doctoral research commonly focuses on a small geographic area or a restricted object of study, partly to make the work less unwieldy, partly to ensure that new ground is researched by specialisation in an area not previously investigated in depth. Perhaps too, it is believed that the research student lacks the familiarity with a broad subject that the established scholar has; only after many years is one qualified to pontificate on general issues. My thesis, then, is upside down for it seeks to provide a general overview of lordly residences of western continental Europe in the fifth to ninth centuries. The project is undeniably ambitious and perhaps over-ambitious. Certainly I have met with 'good advice' from several archaeologists who have suggested that the topic be restricted to a small area of France or Germany, believing the topic to be unmanageable. There are, I believe, several reasons to justify the wide approach adopted.

No general overview existed when I began, although subsequently the work of Streich (1984) has appeared. Although it concentrates on churches connected with princely and royal sites, it also gives a useful introduction to many of these residences. Some regional studies do exist but they are by no means common, and few could stand for the whole in the way that Christlein's (1978) book on the Alamanni is often held up as a valuable introduction to *Reihengräber*. The fragmentation caused by regional studies is further exacerbated by the tendency for discipline boundaries to act as barriers. The result is that renowned scholars may be unfamiliar with work in other areas, countries, or disciplines and some important sites remain obscurely buried away from general knowledge. Thus, Gabriel Fournier, who alone has attempted a synthesis of Merovingian rural fortified sites, mentions the villa Burgus of Pontius Leontius featured in a poem of Fortunatus, but was unaware that Alexandre Nicolai thought he had found and partially excavated it half a century ago (Fournier, pers. comm.). The important site of Pfalzel, just outside Trier, inexplicably features in Böhner's *Fränkische*

Altertümer des Triererlandes only as a place-name, although large amounts of architectural structure remain. This important villa-cum-Frankish monastery has, as far as I am aware, never featured in any important historical synthesis of the post-Roman period.

Regional studies often suffer by not taking account of wider perspectives of their historical object. Dealing with their own small corner of the world, scholars may explain phenomena in historical terms which may seem plausible for their locality, but make little sense viewed in a larger context. Thus Michel Roblin has investigated the Roman and Merovingian remains of several *départements* around Paris and has 'explained' one or two sites as originating as a Roman military strong-point guarding river crossings. However, specialists in Roman military history and the stationing of army garrisons insist that there is no evidence for such bridge patrols, except when connected with forts on river frontiers. Such ideas are easily perpetuated if only 'local historians' are thought capable of understanding 'their' sites. This, I believe, is true of sites in Hessen interpreted as Carolingian military forts. Only by a restricted perspective can local historians fail to recognise that the sites are not uncommon in much of Europe east of Hessen, that they are in keeping with early medieval settlement forms in central Europe, thus making the Carolingian military invasion theory an unnecessary elaboration.

The archaeology-history disciplinary barrier is seen to work its detrimental effects too. In one of the major overviews, that of von Uslar (1964) dealing with 'early medieval fortified sites north of the Alps', 'curtis' is defended as a heuristic term which might be applied to certain archaeological sites enclosed by substantial earthworks. Textual evidence is not consulted for evidence concerning the architecture of Carolingian villas and *curtes* and for evidence of their possible fortification. Historians argued that many of his enclosed sites would not have been called *curtes* by Carolingians and that many that would have earned the name would not have been defended by ramparts. Only four years later did von Uslar bow to their arguments and dropped the term as an archaeological type. The archaeologist's book is full of typologies and categorisations based on morphological form but is thin on the social and historical analysis of these sites.

One result of the wide-ranging approach adopted in this thesis is that the better-known and better-excavated sites predominate. There is little point in demanding a comprehensive corpus of possible sites from this work, for the strength of the approach is that we can concentrate on sites that yield sufficiently good information that useful historical analysis is possible. Too often the creation of a regional corpus entails the expenditure of almost all energy on collating

evidence of stray finds, of dubious dating evidence, and concentrates almost exclusively on sites with visible traces at ground level, which generally means banks and ditches; there is little time left over for historical interpretation.

Although it was primarily through a literature search that I came upon most of these sites, I have visited many discussed in the thesis. Before beginning my doctoral research I lived for six months in Bordeaux and visited many of the southern French sites then or during six weeks of field work in the summer of 1984. I had an opportunity to visit most of the German sites while I lived in Freiburg for twelve months in 1985-6. Below is a table of all the sites in the text that are discussed in depth under their own rubric. I visited all those marked with an asterisk some time between 1983 and 1986.

| | | |
|----------------------|--------------------------|-----------------|
| Aachen | * Gelbe Burg | Reccopolis |
| * Alise-Sainte-Reine | Geneva | * Ronzières |
| Amboise | * Glauberg | * Runder Berg |
| Athies | * Gronauer Altes Schloss | * Saint-Floret |
| Bodman | * Höfe bei Dreihäusen | * Sainte-Odile |
| * Büraburg | * Hünenkeller | Samoussy |
| * Burgus | * Ingelheim | Soissons |
| * Camp de Larina | * Köln | Staffelberg |
| Carouge | * Langmauer | * Theodoric's |
| Chalon-sur-Saône | Le Mesge | palace, Ravenna |
| * Chastel-Marlhac | * Marlenheim | Theopolis |
| * Chastel-sur-Murat | * Mediolanum | * Thiers |
| Chelles | Michelsberg | Tholey |
| * Christenberg | * Paris | * Trier |
| * Dourbes | Paderborn | Vilauba |
| Echternach | * Pfalz | * Vollore |
| * Frankfurt | Quierzy | Zullenstein |
| | | * Zürich |

This list does not include numerous early medieval 'hillforts', for want of a better word, which initially were to form the major interest of this thesis. These include: in France: Botalec, Locronan, Radicatel, St. Lô, Puy de Menoire; and in Germany, Amöneburg, Burg bei Caldern, Heuneberg, Hünenberg bei Oberhöchstad, Johannisberg bei Bad Neuheim, Schwalenburg bei Schwalefeld, Zähringer Burgberg. In addition I visited sites that appear as *castra* in the writings of early medieval authors, such as Blaye, Cabrières, Dio, Erfurt, Gordon, Meissen, Saint-Bertrand-de-Comminges, and various sites designated as villas or palaces, including Sélestat, Kirchheim bei Lorsch, Goslar, Quedlinburg, and Tilleda (the last three Ottonian).

A simple visit to each site, giving an immediate feel for the topography and local environment, sometimes sufficed to challenge the preconceived ideas about the site gained from reading excavation reports. Thus Burg bei Caldern, which is

often cited as being the small *Burg* of some nobleman, strikes the visitor as quite unlike the large Carolingian enclosed sites it is generally compared with. Perched atop a very steep hill, the enclosure is minuscule. The only building it could accommodate was not spacious. This I could not see as a typical nobleman's home. While this site could convince me that it was a look-out post, the supposed Carolingian marching camps were often so difficult to find, often tucked away in quiet sheltered little valleys, some without egress, that they almost immediately became, to my mind, manors of rather unimportant local notables. The frequency with which I found German enclosed sites situated so as to oversee the nearby arable fields, even when this meant it was in turn overlooked by higher and more defensible points, helped convince me that the lordly interest in peasants properly tilling fields predominated.

Immediate local topography was an area I was able to offer new archaeological data in the shape of 1:25,000 map comparisons of known royal residential villa sites. Otherwise the only new archaeological information I was able to offer consists of reinterpretations of old excavations. Other new information, such as the discovery of new sites, would have done nothing for the overall purpose of this thesis. It is only sites that have been excavated, preferably well and tied to textual evidence, that have anything to offer; another enclosed site with possible Pingsdorf ware found while field-walking would not have helped.

Rather than relegate the discussion of individual sites to the appendix, I have incorporated them into the text of the thesis. Likewise I have incorporated the early medieval textual evidence into the thesis text rather than as an appendix. I have tried to give discussions of individual sites an appearance distinct from the rest of the thesis, and the close, small type of the quoted early medieval textual evidence should make it easy for the reader to skip over it, should he or she so desire. This seemed to me preferable to the inconvenience of searching the back pages of the thesis to find a page of poems. Moreover I have included the site discussions and the textual evidence within the body of the thesis because I believe them to be essential and integral. I do not see them as extraneous lists of information to be tacked on to the end of my argument gratuitously.

This does make a further demand. The integration of site description and thesis argument is extremely important, for reliance on a little narrative to act as nothing more than a transition from one site description to the next is a recipe for disaster. The recent publication, *The House and Village in the Middle Ages* by Chapelot and Fossier (which concentrates primarily on excavated houses and villages), uses such a format, linking excavation report to excavation report, but

ultimately fails to create a convincing analytic framework. Their central belief is that the ninth and tenth centuries marked the real origin of rural France: a countryside of small nucleated villages. This allows them even further to avoid the necessity of interweaving their discussions of individual sites from the early medieval period into an elaborate and coherent analysis, for the fifth to ninth centuries are thus seen as a formative period during which the mobility of settlement apparently eradicated all trace of Roman settlement patterns. Somehow, because the period was one of flux, no real sense must be made of the changes, it suffices to document them. The arguments of the advocates of villa continuity from earlier this century, Fustel de Coulanges and Dopsch, as well as the recent theories of Agache and Percival, are all discarded. All this is made that much easier by concentrating only on the excavated village plans to hand and by ignoring the negative evidence. Thus there are no questions such as: where are all the rest of the early medieval settlements?, why do almost no French sites figure in all the chapters before the high Middle Ages? The answer to both questions, almost certainly, is because there was a great deal of continuity and many of the early medieval sites that we would like to see are buried under modern villages.

The geographic limits of the thesis – the Pyrenees, Alps, Bohemian forest, Saale and Elbe rivers, and Atlantic – were largely the results of the prosaic demands of ability to read foreign languages, which has largely limited the research to works in English, French, and German. Fortunately much of the useful research has been published in these languages, including works covering areas where none of these languages are the native tongue. Benelux countries regularly publish in French or German, east German scholars frequently present overviews of work done by their Socialist and Slavic neighbours, while the British School at Rome is the medium through which Italian material is sometimes available, as well as independent British excavations in Spain and Italy. It could be said that the boundaries also coincide roughly with Frankish political hegemony. Any attempt to justify such quasi-ethnic/political boundaries, however, must be avoided. At the beginning of the period investigated, Gaul had much closer similarities to Spain and Italy while at the end, northern Gaul and German Franconia had more in common with one another than with either Mediterranean country. For this reason parallels for Gaul drawn from Italy and Spain are restricted to the first half of the period. For the eastern limits it could be said that they roughly coincide with the Slavic regions, although I do not accept that the 'ethnic' differences had any fundamental effect on the historical conditions of the areas on

either side of the linguistic border. The main historical political development of the period covered was the extension of Frankish hegemony over territories east of the Rhine, territories which had remained beyond the confines of the Roman empire. The geographical limits have been chosen to coincide with the extent of Carolingian incorporation of this 'Germanic' territory. The expansion was in part enabled by and in part cause of the increased social complexity and amount of hierarchical exploitation in this region. Saxony remains largely beyond the scope of this study for the same reason that it gave Charlemagne so much trouble to conquer, unlike Lombardy. In Saxony, as among the Slavic tribes further east, there was a multiplicity of local leaders, political authority was fragmented, there were less highly-developed forms of exploitation. Thus the eastern limits, following the border of the early Carolingian empire, also coincide with the area of greater social complexity, of more developed lordly exploitation.

The chronological starting point was easily set at the fifth century because one of the main themes of the thesis is the development of the post-Roman period and to what extent the villa tradition continued. The mid-ninth century was chosen as a terminus somewhat more arbitrarily. The amount of information derived from the late ninth century onwards, both documentary and archaeological for the geographic area covered, becomes greater, and already there was sufficient material for my thesis. Moreover historical overviews often end the early Middle Ages with Charlemagne's grandsons and begin the next 'phase' with the mid-ninth century. This is primarily the result of the collapse of Charlemagne's empire and increasing subsequent political fragmentation; histories tend to be political narratives of kings and battles. The break was more political than social, economic, or intellectual. At least from the mid-tenth and probably as early as the mid-ninth century, there were new architectural developments which would ultimately lead to the motte and bailey castles of the eleventh and twelfth centuries. And architecture, after all, plays a central role in this thesis.

The thesis concentrates more on the earlier period than the later. There are four chapters dealing with the fifth to seventh centuries and only two dealing with the eighth and ninth centuries. This does not reflect the amount of evidence, quite the reverse. The earlier period is much less well known and elucidation of those darker centuries has been the major goal of this thesis. The better known, better researched Carolingian period is treated more cursorily precisely for that reason. One might even say that it has been included to help bracket that enigmatic period between the far better known centuries of Roman villas and the increasingly comprehensible era of the Carolingian *curtes*. In short, a picture of

the Carolingian villa could be said to be given in order to sketch something between it and the Roman villa: the Merovingian noble's residence.

These geographic and chronological distinctions are combined to form the major divisions of this thesis, entitled Gallia, Merovingia, Germania, and Carolingia. By Gallia I mean the area of the former western Roman empire north of the Alps and Pyrenees, throughout the whole of the early medieval period, from the fifth to the ninth century. Chronologically contemporary, Merovingia and Germania were not simply geographically distinct, but also socio-politically dissimilar. From the mid-eighth century, thus coinciding with the Carolingian period and the extension of Frankish hegemony over Germania, the two areas were brought together politically and brought much closer together socio-politically. The Germanic portion of the Carolingian empire did remain somewhat more, shall we say, backward. Mints were late in being established and a monetised economy was naturally later in developing.

The primary task of this thesis was seen to be the collection and analysis of the archaeological and literary evidence of noble residences, with an aim to present a coherent picture of their major characteristics and how they may have changed through the fifth to ninth century. To that end I have largely had to devise my own methodology for the early period. For the Carolingian period much of the groundwork has already been done by German scholars. Thus chapter six, which is subtitled gazetteer, is largely an English summary of some sites in an increasingly large German corpus. It is, however, no straight translation of work already done. All site descriptions take into account the most recent work, some of which seriously alters previous views of the excavated remains, and throughout I have included my own thoughts and sometimes new interpretations. Such a task of collection necessarily must touch on the theoretical grounds for distinguishing the architecture of 'nobility' from the rest of society, especially east of the Rhine before historical documentation solves the problem. It is not intended to enter into the debate about the 'origins' or the nature of such nobility, although I believe that the subject is in great need of revision.

Part of the primary task of collection and analysis involves the investigation of how modern ideas about the sites have developed. Therefore during the course of almost every chapter historiography will feature. In particular, some of the ideas derived from German historians of the 'Romantic' period in the middle of the last century (often referred to herein as the 'Germanic school') have had lasting effects, nowhere more so than in Marxist history. Predictably, sites on German soil that are effectively prehistoric suffer most acutely from an accretion of historical assumptions and will need the longest historiographic discussion.

Due to the sheer amount of empirical work necessary to provide a general picture of elite residences, I have not been in the position to set myself any one particular historical 'question' to be answered by this thesis. Nevertheless, a few major historical themes are developed in the light of the material treated here. One is an attempt to discuss the development of post-Roman to Carolingian economy, and the Pirenne theory is approached from a different angle. Hodges and Whitehouse (1983, 86) suggest that a comparison of royal palaces with the imperial past reveals how far the urban decay had gone. In fact, the quality and quantity of data is not sufficient to demonstrate satisfactorily whether Merovingian residences or Carolingian residences were 'better' or 'worse', so that, even if one accepted Hodges' and Whitehouse's claim it would be impossible to date when the economic collapse had taken place. In the next chapter I attempt to show that, no differently from Pirenne, Hodges and most other historians and archaeologists have seen trade as being of central importance to the economy and necessary for urbanism. I argue that production and the social relationships that controlled and exploited it was much more important. This was the basis of lordship and of nobility; this was achieved through the agricultural estates of the *potentiores*. This chapter may seem to dwell too long on aspects of settlement other than the residences of *potentiores* and particularly on urban sites instead of the rural manors of the elite, which otherwise make up almost all the rest of the thesis. Although they may not be the best, there are two reasons for this. Firstly, I felt that my argument that towns existed and were the result of widespread exploitation of rural estates, if accepted, was perhaps the best way of arguing for a rural countryside dotted with Merovingian villas instead of Chapelot and Fossier's ephemeral settlements of squalid little huts inhabited by peasants. Secondly, I hoped to emphasise that, just as in the Roman period, nobles had town houses, although much more work is needed to collect the information that is available.

The study of houses cannot be undertaken without at least some awareness of social history. Although forms of personal dependency and the nature of power, authority, and social domination make only occasional appearances in this thesis, my thoughts on these questions were ever present as I wrote. Thus I would argue that Chapelot and Fossier (1985, 122) are wrong to try to give their functional history of *Grubenhäuser*. It is hardly surprising that 'the functions of sunken huts are complex and call for varying explanations.' But the answer is unlikely to be found in rainfall patterns, soil types, and tree cover. In Slavic regions in the sixth and seventh century there was a more egalitarian society than that of Ottonian Germany. In some Slavic areas it appears that everyone lived in *Grubenhäuser*.

Nowhere was the amount of exploitation and social differentiation likely to be greater than at the Ottonian palace of Tilleda, where there were no doubt servants little better than slaves. It seems to have been they who lived in the *Grubenhäuser*. Perhaps the *Grubenhäuser* at Brébières were the dwellings of slaves attached to the royal estate at Vitry-en-Artois. A relationship with slavery may explain the mobility of *Grubenhäuser* in general. Unlike the continuity of tenements over the centuries that recent urban excavations have shown us, slaves lacked the customary rights to till land in the way that even the most servile villeins did in medieval England. The mobility of *Grubenhäuser* might be a reflection of the movement of slaves to different parts of an estate or to different manors altogether. If most *Grubenhäuser* were similarly slave quarters, then their disappearance in western Europe during the late Carolingian period might have nothing to do with the end of settlement mobility as Chapelot and Fossier claim, and may have more to do with the gradual disappearance of slavery.

Other historical themes have already been mentioned, namely the continuity of Roman settlements and practice within the confines of what had been the Roman empire and the question of Frankish imperialism and its effects on Alemannic, Thuringian, and Saxon territories. Incorporation within the Frankish empire ought to have similarly affected Breton territories, but the situation there is far from clear. Finally, I hope that this thesis contributes to the social history of the Merovingian and Carolingian period. Here is where an interdisciplinary, archaeological, and historical approach is most valuable. It deals with the material world of the past but is more concerned with the social actors who created and lived in it than with the inanimate objects themselves.

Introduction to the Study of Villas, Palaces, and Estates

Although French historians were already collating evidence for early medieval royal estates in the eighteenth century, there are few studies prior to this thesis that have been much concerned with the physical appearance of early medieval nobles' residences.

The buildings and the lands composing a villa were divided into four groups. The first included the mansion of the master, the *casa dominica*, with its various buildings of all kinds. In front of the mansion there was a court, which played a great part in the life of the community, and was enclosed by walls to which the buildings for the household staff were contiguous: beyond which came a kitchen garden, a small park and an orchard. This first group was surrounded by a wall or a moat enclosed by a palisade.

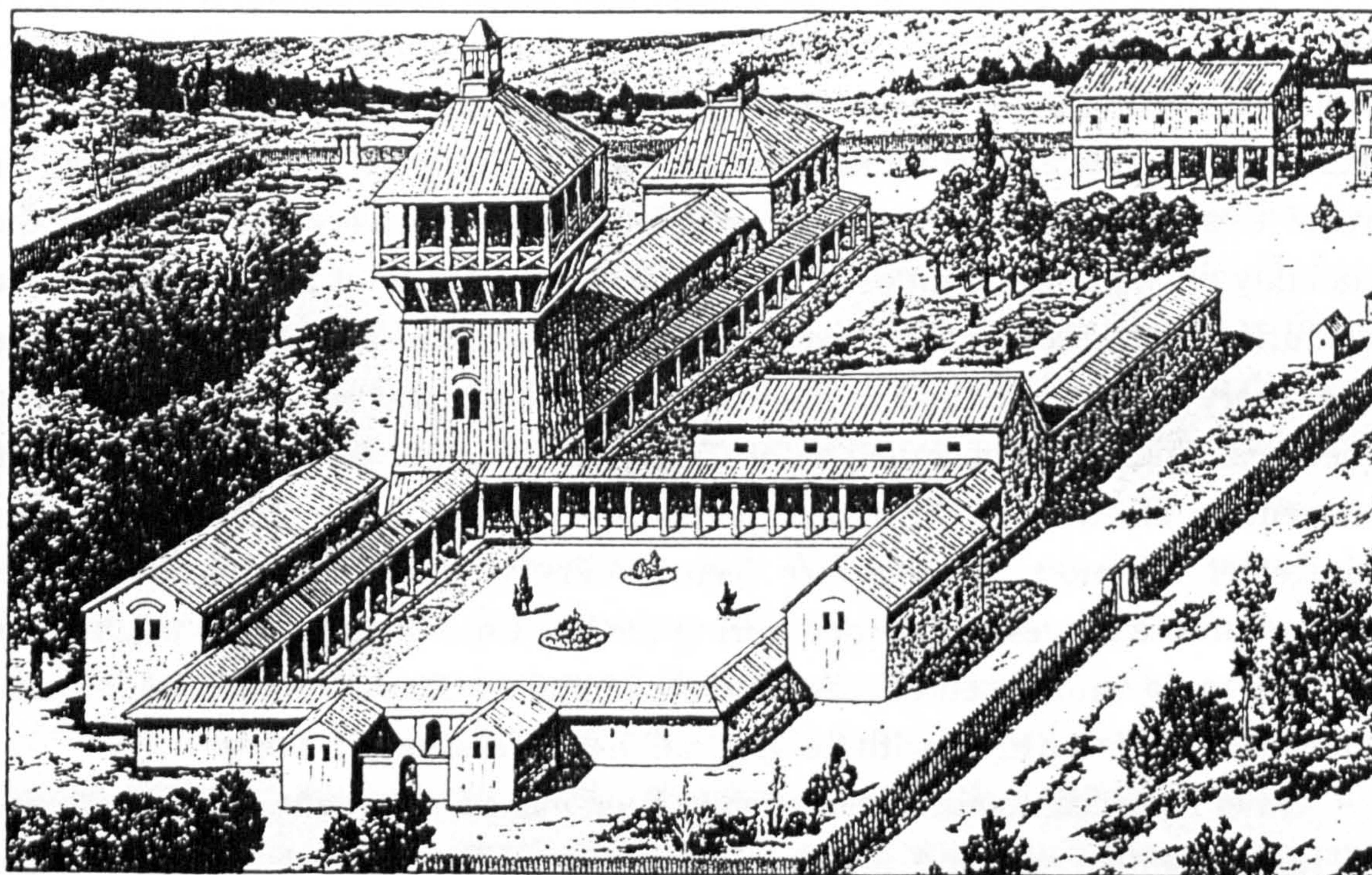


Fig. 1.1 Hypothetical Frankish villa (by K. Stephani).

Funck-Brentano (1927, 289) depicts a believable Merovingian villa, but one which has been conjured partially from the imagination. As an historian, the legal, economic and social aspects appeared paramount, so that the judicious invention of the physical setting must have seemed perfectly honest practice to Funk-Brentano. Indeed, Robert Latouche (1961), although explicitly conscious of how laconic are the surviving Merovingian documents, treads a similar inventive path when describing a hypothetical Merovingian villa. Alas, archaeologists are not allowed such freedom. If they were, they could produce such pleasing reconstructions of a Frankish villa as did Stephani (1903) (fig. 1.1).

'L'habitat mérovingien est mal connu' wrote E. Salin in 1950 (p. 410). By 1975 the situation had changed so little that Guy Fourquin (1975, 317) was still able to write *'la villa est mal connue.'* Archaeologically, the study of Merovingian villas is still in its infancy. The situation is very reminiscent of the British position three decades ago, when Anglo-Saxon timbered halls were postulated on the strength of documentary evidence and foreign analogies but archaeology had only uncovered those at Yeavering (Radford 1957). One lesson the analogy should teach us is that it is incomparably easier to find something if one knows what it looks like. *Grubenhäuser* have long been easily recognised by archaeologists, not so timber halls. While Merovingian farmsteads are generally assumed to be timber-built, the number of halls known is small. It is of great importance that we

understand or at least produce theories to explain the gaps or absences in the archaeological record. These theories may in turn contribute to our knowledge of the past, for they contain explicit implications about it. Thus the absence of fifth and sixth century villas from the archaeological record could mean they were never there, evidence for depopulation perhaps. Or they were there but cannot now be found. Are they just hard to spot, unlike Roman villas in stone; were they all built of timber posts? Are they hard to find because they have been removed by continuous occupation through the Middle Ages; is their absence evidence of site continuity?

There is some archaeological evidence, and an attempt will be made here to investigate the little which has come to light, but the state of our knowledge at present is heavily dependent on documentary evidence, place-name studies, and topography. The lack of any large body of any single type of settlement evidence has meant that an inter-disciplinary study has been forced upon students of the post-Roman period. Yet studies like *Civitas und Palatium* of Carlrichard Brühl are not common. The opening sentence of his book (1975, 1) declares that for many historians topography is no more than a third rate aid to history. The prejudice that archaeology is the handmaiden of history clearly transcends national boundaries. The rarity of such inter-disciplinary work is perhaps seen in the warm reception and general acclaim that Gabriel Fournier's work on the Auvergne received among French scholars; it was recognised to stand alone. The work of May Vieillard-Troiekouroff (1976) show just how well documentary, archaeological, and place-name evidence can be used together.

Although it is greatly to be hoped that future studies will wed history and archaeology as successfully, there are serious obstacles. It is not coincidence that Brühl, Fournier, and (perhaps) Vieillard-Troiekouroff are all historians first and archaeologists second, for most of the documentary evidence they use remains inaccessible to those who cannot read Latin competently and confidently. The documentary material can very roughly be divided into four categories based on content: histories and annals, saints' lives, letters and poems, and legal material. The different sources have received uneven treatment in study and translation, so that the *Historia Francorum* of Gregory of Tours, the *Chronicle of Fredegar* and the *Royal Frankish Annals* are available in English translation, while almost none of the Merovingian saints' lives are available, even in French translations, with the exception of the works of Gregory of Tours recently translated in three volumes by Edward James and Raymond van Dam. That this presents a real bias in the documentary evidence used is probably reflected in the frequency with which Sidonius Apollinaris and Gregory of Tours (*HF* only) are quoted. Pierre Riché

(1976, 161) claims it is necessary to turn to hagiography, long disdained by historians, to penetrate royal Merovingian palaces. Although he claims that they yield no description of palatial buildings, '*les sources hagiographiques ne nous donnent pas malheureusement la description des bâtiments palatiaux . . .*', before dismissing them, we should note that Riché (1976, 161) also claims that Gregory of Tours '*ne donne aucun détail qu'attendrait l'archéologue*', which I will show to be not entirely correct. How much information is in fact lost to archaeologists who are unable or who have not the time to search thoroughly through a dozen or more volumes of the *Monumenta Germaniae Historia*, is impossible for me to assess. To the four categories of written, textual material, might be added pictorial representations of buildings found either as manuscript illustrations or as paintings or mosaics on church walls.

Loosely we could say that specific information which written sources might offer can be divided into three categories: 1) the detailed description of dwellings, the building or roofing materials, number of stories, distinctive rooms for specific activities (eg. sleeping, cooking, entertaining, bathing), perhaps lay-out, size, existence of enclosures or a multiplicity of buildings and their functions; 2) the extent of an entire estate, the form of its lay-out, uses to which different parts might be put; and 3) the details of ownership.

Annals, histories, and saints' lives are better suited to the first category of information, providing anecdotes of particular political importance, such as the assassination of kings involving knifings in courtyards or strangulation in bedrooms, or saintly miracles such as visions on tiled roof-tops or cures in humble peasant huts, in which such information is supplied quite incidentally and is thus arguably less prone to purposeful distortion by the author. Vieillard-Troiekouroff (1976, 12) notes that '*Gregoire ne décrit aucun palais ou villa comme il décrit certains sanctuaires*.' It is true that Gregory did not explicitly describe any palace or villa (although Sidonius and Fortunatus did), but it is not true that he gives archaeologists no details of interest as Riché says. The fragmentary details must be compiled and used to build composite pictures. This is precisely what Dolling (1958) did, using the barbarian lawcodes. To this end one might also try to use the pictorial representations of buildings in art. But they are less helpful than one might expect, being highly stylised with odd perspectives and simultaneous 'cut-away' views, so that some conventions have only comparatively recently become comprehensible (Lampl 1961).

The remaining two categories of information, concerning the extent of estates and their ownership, are best served by the legal sources. Walter Goffart (1981) has accustomed us to the fact that Merovingian polyptychs did once survive and

probably thrived, although none are now extant. Gasnault (1970) discusses how financial documents of St. Martin's at Tours were used. We possess some 300 Merovingian charters: acts of sale, gift, exchange, and wills and some 400 formulas for the production of charters, which may once have numbered in the thousands (Fustel de Coulanges 1889, 113-5). Fustel de Coulanges's estimate must be very conservative. The existence of formulas for charters of every type implies a sort of mass production. More empirically, Margaret Weidemann (1986, 115-20) reveals that, contained within the text of Bishop Bertram's will, there are explicit and probable references to several dozen other documents including royal precepts, letters, charters, titles, and manumissions, none of which survive. The legal documents have unfortunately been as neglected as the hagiographic material at the hands of translators. Wills have been used by historians, best-known being the testament of Bishop Bertrand of Le Mans which has only recently been re-edited and translated (Weidemann 1986), but most of the other material remains in Latin and when studied it is primarily questions of property ownership and more especially its relationship to the monarchy which are asked (Jones *et al.* 1957; Levison 1932; Meyer-Marthaler and Meyer-Marthaler 1946). Topographic information about the estates contained within the wills remain of interest only to local historians and *Lokalgeschichte* (Brühl 1975, 1). Merovingian charters were extensively studied by Fustel de Coulanges (1889, 172) and their contents were very familiar to him as he picturesquely put it: '*J'ai lu tous ces documents, non pas une fois, mais plusieurs fois . . . et d'un bout à l'autre.*' His pronouncements on Merovingian estates, if repetitive, were authoritative and have no doubt curbed historians' interest in returning to the sources. De Coulanges (1889, 231) claimed '*l'étendue de ces villae . . . nos chartes ne l'indiquent jamais.*' British archaeologists and historians, accustomed to Anglo-Saxon charters being frequently supplied with entire estate boundaries, will be surprised to learn that similar charters did not exist in Gaul. In actual fact, some few charters do exist which detail the boundaries of donations. It would appear from the secondary works that these few charters are all that are known, so frequently do they appear as the only examples. Thus the charter of Tresson appears in de la Roncière *et alia* (1969), Bouton (1962), and Latouche (1961). That this question has not been exhaustively researched is shown by Bouton's (1962) plagiarisation of Latouche on the meaning of the charter of St-Calais. It is revealed by the fact that no plan seems ever to have been attempted of the boundaries given therein. Likewise, little has been made of another charter with boundaries, that of the villa Malmedy of Stavelot monastery. That it has no doubt long been known to local historians is of little help when it does not appear in general works (recent

German work on the monastery and the area has been unavailable to me; I presume it deals with the boundaries). In short it must be said, there is immense scope for further work in extracting evidence from Merovingian documents to help understand the forms in which Merovingian villas existed.

The main thrust of legal and social history has been aimed at villa organisation and peasant dependency. As Verhulst (1985, 11) notes, 'the problem of continuity between the great estate of late Antiquity and the so-called classical *régime domanial* of the Carolingian period . . . already preoccupied nineteenth-century historians.' The debates on the origin of the classic Carolingian estate revolve around the size of early medieval villas, the division of demesne and tenanted land on a seignorial estate, the forms of servile labour, renders, and obligations, the relationship of the *mansus* to the *villa*. They are occasionally interspersed with odd historical explanations such as Charlemagne's 'agrarian politics' or some natural demographic and economic trajectory. Estate records and polyptychs form the central source of evidence, such as those of St-Germain-des-Prés, Lobbes, Lorsch, or Prüm. And studies of this material have produced a vast literature (Verhulst 1985 for bibliography). But because they date only from the ninth century, the end of the period covered in this thesis, they are of limited value to us. They cannot be ignored, however, for they are extremely valuable for the evidence they give about the estates into which Merovingian and early Carolingian villas would eventually evolve. Moreover the very question of evolution structures most of the things said about earlier estates in the scholarly literature.

Of all these uses to which the textual evidence might be put, the most immediately relevant to the archaeologist is also the one that has been most ignored by historians. I therefore devoted much of my time and efforts in recording of all references I found to specific words, such as *castra* and *castella*; *civitates*, *urbes*, and *vici*, and *villae*, *curtes*, *domus*, *metatio*, *casae*, and *fundi*. This I did systematically for all the works of Gregory of Tours through three media: the original text in the MGH with concordance, English translations, and M. Weidemann's study. I also systematically went through Fredegar's *Chronicle* and the *Royal Frankish Annals*. I also used M. Weidemann's study of Bishop Bertram's will to look closely at that text, and browsed through many others, including most of the royal Merovingian donation charters in the original; the Salian, Ripuarian, Burgundian, and Lombard lawcodes; some of the poems of Fortunatus, Ermold's *Poem to Louis the Pious*; correspondence of Alcuin, some of Boniface, and Lupus de Ferrières; the *Dialogues* of both Gregory the Great and Severus; and Paul the Deacon's *History of the Lombards*. To this list can be included

a number of assorted medieval texts, such as capitularies, brought together in various anthologies – documentation of paganism, of Carolingian administration, of general aspects of early medieval culture – in translation (English, French, and German). I also read through many saint's lives and lives of nobles, including: St. Adalard by Radbert, St. Boniface by Willibrord, St. Columbanus by Jonas, St. Epiphanius by Ennodius, St. Germanus by Constantius, St. Honoratus by Hilary, St. Lebuin by anonymous, St. Leoba by Rudolf, St. Martin by Severus, St. Radegunda by Fortunatus, St. Severinus by Eugippius, St. Sturm by Eigil, St. Willibrord by Alcuin, Charlemagne by Einhard, another by Notker, Louis the Pious by the Astronomer, and Wala by Radbert. In all this reading I was on the look out for any information concerning architecture or fortifications or anything out of the ordinary concerning the use of the specific terms enumerated above. Of course, I also noted any useful references in secondary sources to statements made in medieval texts that I happened to come across.

Medieval terminology was not nearly as precise as our archaeological vocabulary and although a good argument could be made for abandoning the terminology of the medieval sources for an artificial one which reflects our own classificatory system, the usage in this thesis has not been regularised and largely reflects that of the documents. *Domus, villa, curtis regis, fiscus, palatio*, or even *castellum*, what is meant by the medieval sources? Diepenbach, according to Brühl, found eleven different meanings for the word *palatium* as used by the documentary sources, while Dopsch believed that the above list of terms could be treated as synonyms. In the case of *palatium*, the medieval sources broadly meant three things, the people whom we would call the court, the geographical site, and the actual buildings. By palace, *palais*, or *Pfalz* we mean particularly the latter and also by extension, the whole palace complex and thus, just as medieval writers, the whole site. Further nuances incorporated into the term are those of ownership (royal in most cases, although episcopal or ducal palaces are also recognised) and residence. The latter is the most important attribute. A royal estate which never experienced royal residence cannot be considered a palace. Furthermore, what was a palace in one source need not have been a palace in another, nor indeed did individual writers remain consistent. Paderborn was designated *locus, oppidum, villa, castrum*, or *civitas* and somewhat later, *curtis* (Balzer 1979, *passim*) depending on the source and although *palatium* is the one term missing, palace is the term used by scholars today to describe Carolingian Paderborn. The *Annales Bertiniani* show a marked tendency to use *palatium*, while the *Annales Fuldensis* preferred to

restrict the term to the very important residences of the Carolingian kings. The usage of the Fulda annals shows another tendency quite clearly. Just as we would do, without creating a rigorous definition for palace, the sources tend to restrict the term to those sites, such as Clichy, Quierzy, Aachen, Frankfurt, Ingelheim, and Compiègne among others, at which royalty spent longer periods, held more meetings and on which they indisputably lavished more money and care.

Changes in the contemporary usage of terminology in reference to royal villas are quite interesting. Gregory of Tours reserved the title of *palatium* for perhaps only two buildings or sites, those of Metz and Vienne, both urban. Everywhere else the terms *villa* or *domus* suffice, when indeed the place-name did not simply stand alone. In the *Chronicle of Fredegar*, the same avoidance of the term *palatium* is to be found, except to describe the court, again like Gregory. Only in reference to Clichy is the term used, and then only to distinguish the royal apartments from those of the referendary. Fredegar's continuator used *palatium* for Bourges. Otherwise the common term had changed to *villa publica*. It was in the Carolingian period that this term grew popular. In the *Annales regni Francorum* the term *villa* is preferred to *palatium* until 794, then to 806 *palatium* becomes more common, after which both become equally infrequent as the place-name alone sufficed in 71 of 84 cases. The charter evidence reveals the same tendencies as the other documents, namely the sparing use of the term *palatium* in Merovingian charters, becoming more common in those of the Carolingians. Interestingly it was shortly before Charlemagne's imperial coronation that we find the adoption of a Lombard custom to refer to the royal palace, both villa and court, as *sacrum palatium* (Bullough 1985). Thus one possible development to pose as a hypothesis from the documentary terminology is that Carolingian royal residential architecture sought to distinguish itself more and more from that of the rest of society.

Palaces were in essence no more than the most important royal villas. Carolingian kings could have hundreds of estates or villas, but only a few dozen palaces. Such palaces, however, were as much centres for agricultural production as were other royal villas. Because of this agronomic characteristic the word *villa*, already embodying this concept, will frequently be used throughout this thesis; the documents themselves use *villa*, *curtis*, *fundus*, *praedium*, or *fiscus*. The term estate is used, particularly in the sections devoted to the extent of the estates, with a more marked emphasis on the agricultural aspects of the villa.

Perhaps separated from direct supervision of agricultural production were urban palaces, although not necessarily so for in many cases they were probably served by estates lying near-by. There was, for one thing, no distinct term by

which to distinguish them from villa palaces, although we have just noted that Gregory of Tours may have implied a difference by reserving *palatium* for two urban sites. A particular palace, likewise undistinguished by contemporaries with an exclusive term, is described by the term *Klosterpfalz*, which has no convenient English equivalent. This was a royal palace or royal apartments constructed within and by a monastery. The earliest known such palace dates to Charlemagne's reign at St-Denis. These *Klosterpfalzen* were built by the monastic houses themselves and in staying there kings relied on the hospitality of the monastery. Brühl (1968) shows that, before the *Klosterpfalzen* truly came into their own in the reign of Louis the Pious, the anecdotal evidence suggests that when kings did stay in cities and visited bishops, it was not as guests, but that they resided on their own property, with the implication that their urban palaces were served by agricultural estates.

It is worth noting here that I distinguish between villa and *villa* during the course of the thesis. Italicised, *villa* is used to mean the concept found in texts. Upright, villa is used to mean my own understanding of a villa in general.

Of a more archaeological nature, documentary sources may offer a chronological liferaft for topographical studies, when a physical site is equated with a documented place-name. Such topographic studies, alas, have yet to be seriously undertaken in France (the situation we will see is different in Germany) although the groundwork has already been partially completed. There has been a long historical tradition of collating the evidence for royal estates and the gazeteers so compiled prepare the road for topographical studies.

In 1881 K. Plath called for an exhaustive literature search to establish a complete list of Frankish, thus Merovingian and Carolingian, royal villas and pertinent information relating to them. Already, in 1709 J. Mabillon and M. Germain had produced a *Commentarius de antiquis regum Francorum palatiis*. The definitive list has been sought ever since. Perhaps the most extensive attempt ever was that of J. Thompson (1935), who concentrated on Carolingian estates, but a number of mistakes are to be found in his work. Nearly a century after Plath, Brühl (1968, 12) recognised that for the Merovingian period such a definitive list would be an invaluable research tool; he thus effectively discarded the work of Bergengruen (1958). An abortive attempt was made to incorporate in this thesis a provisional list of Merovingian royal villas based primarily on the work of Bergengruen (1958), with corrections based on Thompson (1935) and additions based particularly on the testimony of Gregory of Tours, my own interpretations, and those of M. Weidemann (1982). The greatest difficulty in producing such a list stems from the uncertainty of whether charters are genuine or not. Brühl notes

that the collection by Pertz is so unreliable as to be useless for research purposes. The situation cannot change until the Merovingian charters have been thoroughly reinvestigated. Even once such a study had been undertaken, a comprehensive list would only represent a very partial picture of royal estates. Firstly, not all charters have survived. Those that have are largely those of estates originally donated to the Church, and the secular donations that do survive recorded in charters were those subsequently donated to the Church. Moreover, only the charters of a few religious houses are still extant, and these are demonstrably only a selection of the total they once held. Secondly, it was only the estates which the kings alienated which are recorded. Precisely those most important royal villas which remained in the kings' hands are missing from the charters, at least as donations. The lists produced for the following chapters, therefore, are lists of villas and palaces which Merovingian and Carolingian kings are recorded as having *visited*. The evidence is derived in large part from contemporary histories and from the witnessing of charters, for it was primarily at royal villas that donations were recognised and witnessed, whether or not the charter was actually produced there.

Using the dates recorded in the charters, historians, of whom Ewig (1963; 1965) may be considered one of the pioneers, have been able to build up itineraries of kings, tracking their perambulations through their kingdom from estate to estate. Gauert (1965c) produced an itinerary for Charlemagne, but the work of Brühl (1968) will long remain the definitive work for some of the continent during the earlier Middle Ages. This approach has aimed at answering questions such as, to what extent did the king 'live off his own' and to what extent did he demand and rely on hospitality, or did the king have a 'capital' – a *sedes* – to which he frequently returned and where contemporaries might expect to find a functioning administration even in his absence. Changes in the pattern of estates visited through time might then be interpreted as reflecting historical developments. The clearest is simply the division of the kingdom, but shifts in areas of frequent visitation may imply political troubles in the area frequently visited, while a decrease in the number of different estates visited may imply the impoverishment of the king through continual alienation of his property. It is precisely for this equation of alienated fisc with declining political power that Thompson, with his *Dissolution of the Carolingian Fisc*, is famous.

Such studies must be seen in German scholarship as part of a wider trend in the research into 'German' royal palaces. Its historiography has been documented by H. Heimpele (1965) and only a few notes are necessary here. Already begun before the nineteenth century, the first high-water point is marked by the work of

Konrad Plath, mentioned above. He foresaw a programme which would produce a critical, definitive list of all the palaces, establish their topographical position, collect all the known archaeological evidence, including the question of possible fortification, and relate the information to traffic routes and local and national histories. Heimpel saw it as having foreshadowed the task now set by the Max-Planck Institute for itself. Such a systematic investigation was proposed in 1957. By 1958 the example of Meresburg was offered at a conference by Schlesinger as a model for future work. In 1963 the first volume of edited papers *Deutsche Königspfalzen* was published and in connection with this enterprise two further volumes have appeared, as have a number of regional gazetteers following Schlesinger's model, the records and photographs of Christian Rauch's excavations at Ingelheim at the turn of the century have been lavishly published, and a few dozen monographs by different scholars have been produced, extending the Max-Planck Institute's original research scheme to include *Königshöfe* (e.g. Flach 1976; Gockel 1970; Heinemeyer 1971; Müller-Kehlen 1973; Schmitt 1974).

The work has been dominated by Germans. Already in Plath's day, he noticed that French archaeologists were falling behind in as much that they showed little acquaintance with the German work on Carolingian architecture. The excavations at Ingelheim were part of a programme begun by the *Deutscher Verein für Kunstwissenschaft*, founded in 1908 to produce a series 'Monuments of German Art' as a sort of parallel to the 'Monumenta Germaniae Historica', which launched itself into the research of imperial palaces. Although Aachen, Nimwegen, and Ingelheim were investigated, only the later Goslar, Eger, and Wimpfen were published (Fehring 1987, 11). By Brühl's (1968, 3) time, he could note in his introduction that the topic had scarcely attracted attention in France since 1930. Since then the active work of the Max-Planck Institute has left the discrepancy embarrassing. It is amusing to note the perhaps understandable tone of impatience in the excavation reports of the German archaeologists Plath and Weise, after undertaking excavation on French soil to try to solve the mystery of Frankish palaces. The backwardness of research in France is immediately betrayed by the commonness of statements such as: although first documented in the sixth century, previous occupation is clearly proven by the discovery of paleolithic flints and polished stone axes The question of continuity from Roman to Merovingian times is quite difficult enough to prove conclusively without trying to extend that continuity back a further eight thousand years into antiquity! The need, clearly felt, to include every scrap of archaeological evidence in the discussion, regardless of date, is surely amateurish. The gap between the

quality of German and French research is not to be sought in differential preservation; sites of royal estates in France are today often no more than hamlets and thus not necessarily obliterated by excessive urban activity. Nor have sites in Germany been exempt from continuous subsequent occupation, for such sites as Paderborn or Frankfurt offered little hope of archaeological recovery yet yield more results than most French sites. One result of this imbalance is that there exists incomparably better evidence accumulated from German soil and therefore, almost by definition, these Frankish palaces are Carolingian or later. Again the Merovingians are left in darkness.

One cannot rely on German archaeological research to understand pre-Carolingian villas. French research must be used. It has, unfortunately, tended to be very local in restriction. Roblin's (1951; 1978) work is archetypical of what may be expected. Sites in a local area are gazeteered with such meagre information as: topographic setting; toponymic information (primarily the meaning and original language of the place-names); the dedication of the local church; whether it was ever a parish church and, if lucky, its relationship (real or hypothetical) to other churches and dependancies; the existence of any near-by row-grave cemeteries; other assorted archaeological findings of which only those of the Roman period can be said to be relevant; and such historical information as may be revealed in early medieval documents. Some of this information can be of value as will be seen when the methodology for finding the possible location of such early medieval villa sites is discussed. Some information appears to be collected for the sake of collection; toponymic information, when revealing that the site was marshy, for example, is generally redundant – the toponymic data is generally patently obvious today: it is on a hill, by the river, in a marsh, and so forth.

The knowledge that some seventy per cent of all the Merovingian villa names, as recorded by Bergengruen, had Latin origins, and that almost all with Germanic names were derived from personal names, reinforces the impression that the fourth and fifth century landscape remained largely intact. Studies of place-names in Bavaria and those in the region of Breisach and Konstanz have all been shown to be very fruitful in personal names which can be found in local monastic charters, often as witnesses. This appears to imply a comparatively recent creation of the villas and the estate system (although it is always possible that it just represents a custom of changing farmstead names with new ownership). Bergengruen (1958) shows that this phenomenon is not seen in Gaul.

The patchiness of our evidence means that our interpretations (even more than usual) are greatly dependent on our theories or particular framework of historical analysis. The direction of the thrust of de Coulanges' work is easily

understood in the light of the then contemporary theories. He, quite rightly, was obsessed with unmasking the fabulous interpretations and poor methodology of those of the 'German school', who propounded theories of Germanic collective, primitive, democratic agriculturalists in nucleated villages. This present research is likewise shaped largely by current ideas and prejudices held concerning the dwellings of Merovingian grandees. Beyond the immediate task of collating and interpreting the meagre information that does exist for Dark Age villas and estates, these traditional conceptions and misconceptions must be evaluated. One such, is that with the demise of Antiquity, wood became the building material par excellence, which has been further refined to the opposition of north to south or Germanic to Roman. Another is that *curtis* somehow translates literally as a courtyard and that all Merovingian and Carolingian villas can be assumed to have contained a physical courtyard, preferably enclosed. Yet another is that the Merovingians were no great builders (Brühl 1968, 10), with the result that well-built masonry is interpreted by archaeologists as Roman, while poor construction becomes Merovingian when there is otherwise no dating evidence. We must constantly bear in mind these pervading prejudices when interpreting the work of others. Perhaps of the greatest influence is the line of inquiry which has achieved primacy; namely the explanation of the apparent contradictions of both continuity and rupture with the Roman past. This forms a major topic of chapter three.

Chapter Two

Merovingian and Carolingian Geography

GALLIA

Universal impoverishment, decline of commerce, handicrafts, the arts, and of the population; decay or even retrogression of agriculture to a lower state - that was the final result of Roman world supremacy (Frederick Schodt 1984).

The geography of Merovingian Gaul, despite the title of Longnon's (1878) book, is still to be written. Paradoxically, the ever increasing amount of archaeological evidence from fifth- to ninth-century settlements in western Europe has left the picture in many ways less clear than ever. The conclusions of the recent book, *The House and Village in the Middle Ages*, by the French scholars Jean Chapelot and Robert Fossier are in many ways remarkable. The fundamental argument of the book is that settlements were extremely 'unstable' (mobile might be a better word) following the collapse of the Roman empire, and that they only became fixed and started to grow into the nucleus of proper medieval villages in the Carolingian period. The work concentrates on small rural settlements. More common are works that are devoted to towns, but seldom is the effort made to investigate urban and rural settlements together, to create a unified geographic synthesis. That seems all the more odd, for Chapelot and Fossier have recognised the ninth and tenth centuries as crucial in the formation of the European rural countryside, just as most scholars put the roots of medieval towns in this same period. It is surprising therefore that no one has taken up their thesis of small shifting ephemeral hamlets in the fifth to eighth centuries in order to suggest that here we have the reason why once proud towns were reduced to ill built wooden huts among dozens of tiny stone churches in a sea of masonry rubble: miserable little rural huts could support nothing more than miserable little urban huts.

My argument, however, is that the fifth to eighth century was not a time of shifting rural settlements and that towns did indeed flourish.

Towns

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historical analysis somewhat difficult. First analysis has to be translated into a definition of what constitutes continuity, the weakness of which is immediately apparent in the course of debate: if the two protagonists cannot agree on a common definition, meaningful exchange becomes impossible. Thus Dodie Brooks (1986) devotes a section of her paper on the evidence for continuity in British towns in the fifth and sixth centuries to the intellectual integrity of 'bridging strategies' used by proponents of the continuity theory. She rightly questions for example whether 'intermittent continuity' is not a contradiction in terms.

It is little wonder that Marxist archaeologists in Socialist Europe find 'bourgeois' continuity-discontinuity debates lamentable. They hold that all history is change and this is what must be studied and understood. It is undeniable that ninth-century towns were different from sixth-century towns, which in turn were unlike those of the third century. Our analysis should seek to understand and explain the changes rather than define how much change is permissible to still count as being 'the same'. But then, the danger of sophistry in the continuity-discontinuity debates seldom materialises for most of the discussions centre on whether or not there actually was any settlement in the former Roman towns during the fifth to eighth centuries. Neither is there much debate; there is a fair degree of unanimity that there was little urban settlement.

The Empty Town

Archaeologists regularly assume that from the fifth century, old Roman towns, shrunken to small almost insignificant settlements, were all but abandoned. Or they go further (D. Brooks 1986, 99):

Thus the most reasonable general conclusion seems to be that the principal towns of Roman Britain were deserted by the mid-fifth century, and remained so for at least a hundred years. Where Saxon towns grew on their ruins those towns were new developments on deserted sites, whose standing walls and ruined buildings indicated they had once been towns.

Although it has long been traditional to deny urban continuity in England, archaeologists on the continent similarly offer a bleak picture. Heiko Steuer (1980, 62) says of Köln *infra muros*, 'one cannot conceive of Köln uninhabited enough; archaeologists simply find no trace of the early Franks.' Even in Italy, where it is more widely held that urban life continued after the collapse of the Western Empire, we find that David Whitehouse (1988) suggests that Rome lost ninety per cent of its population during the fifth and sixth centuries and saw another two hundred years of 'stagnation' thereafter. Perhaps nothing demonstrates the generally pictured decline of post-Roman cities better than the graph of town

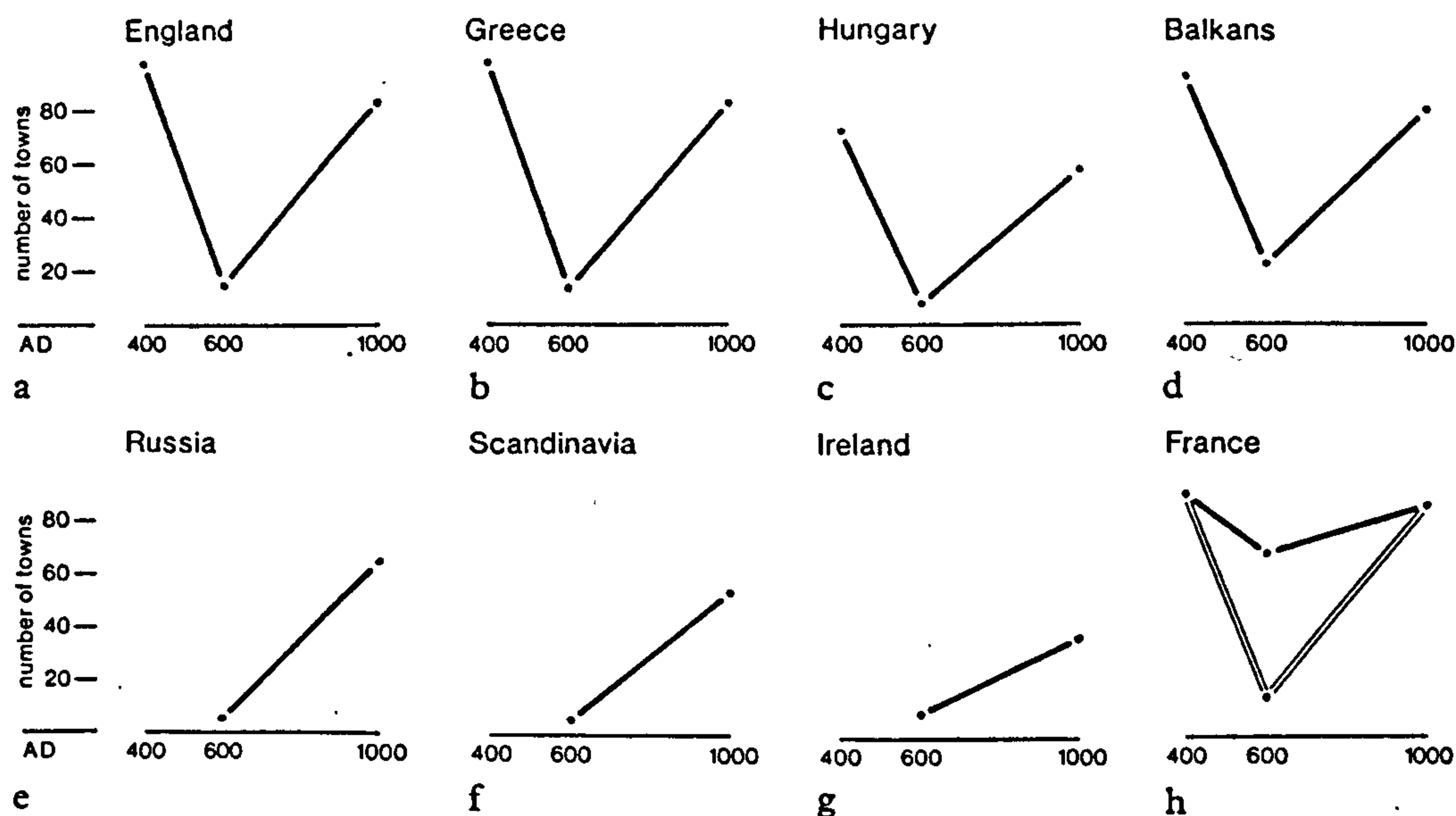


Fig. 2.1 Graphs of supposed decline and rise in the numbers of towns in Europe during the early Middle Ages (after Hill). The graphs are no more than a subjective representation, for the problems of sources and definitions are all but insurmountable. Note that despite Hill's concern to show that urban existence was extinguished during the early Middle Ages, he was forced to reveal a marked continuity in France.

numbers produced by David Hill (1988) (fig. 2.1). The figures, according to Hill, were drawn from various individual scholars working on urban history for the particular period of their interest. There was no systematic unity of approach, but a reflection of general consensus. In his own words (p. 12) it demonstrates 'the decline of towns from the Roman period to the "Dark Ages" and then the resurgence up to the tenth or eleventh century.'

Today we rely largely on archaeological evidence to tell us about the extent of occupation in post-Roman towns and the activities that occurred there. But there are problems and biases in this evidence. If the problems are better understood, it may lead us to a better understanding of the negative evidence. Thus it may be that archaeologists regularly underestimate how partial our evidence is. Martin Biddle (1976, 116) was once able to suggest that 'London, within the 133.5 ha. bounded by its Roman wall, can never have approached the 240 ha. of contemporary Dorestad.' The recent discoveries of Anglo-Saxon London reveal that the previous lack of evidence was *not* because Anglo-Saxon London had been insubstantial.

There is a particular problem with the evidence of stray finds, and their absence, in continental towns. Steuer puts his finger on one of the problems at Köln without quite recognising it, for at Köln all the finds come from cemeteries, which lie *extra muros*. Apart from the episcopal church, we know of no churches that lay inside the town walls during the Merovingian period. Quite simply, the

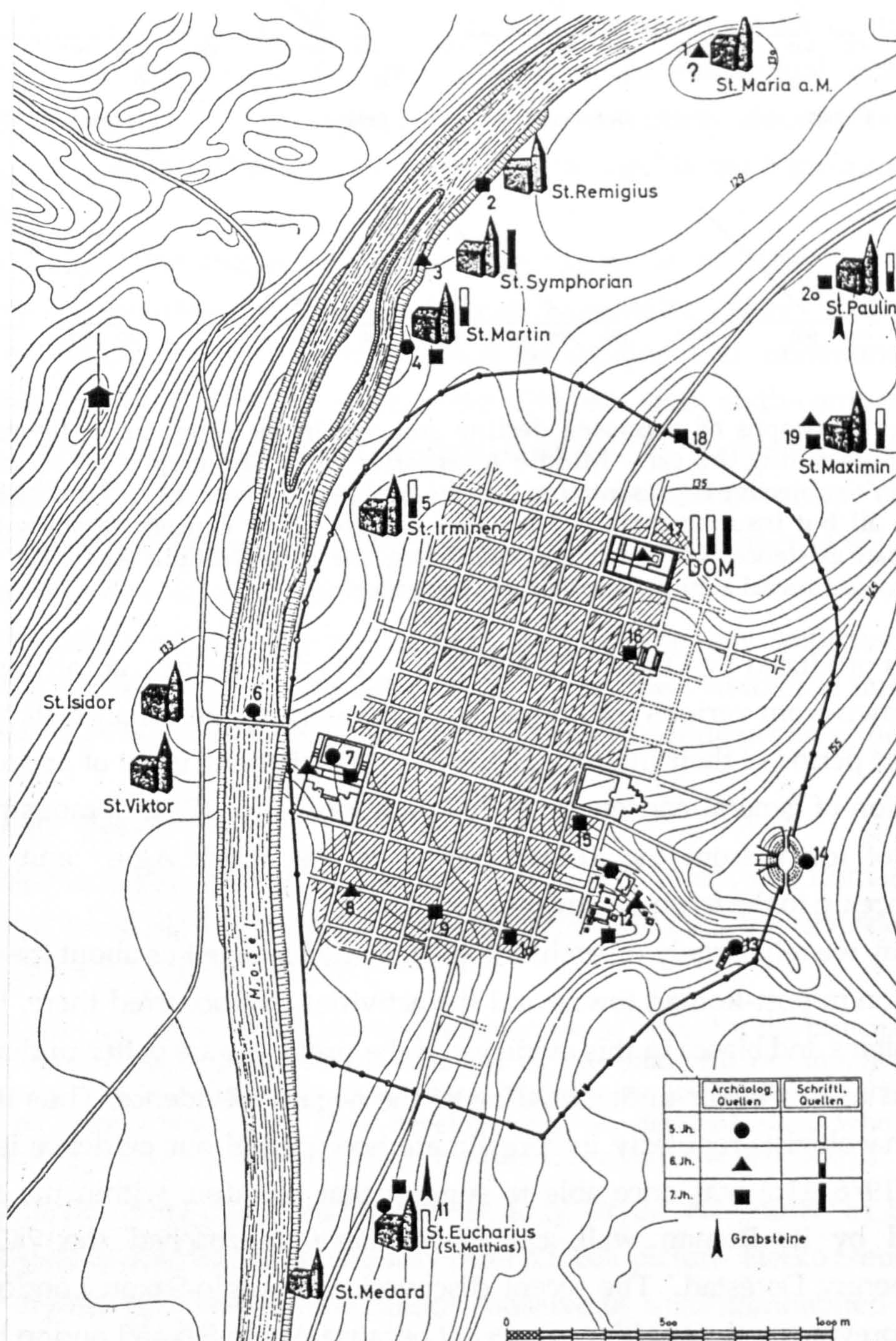


Fig. 2.2 Plan of Trier, showing early medieval churches and archaeological finds, both excavated and stray (after Schindler). Note the overwhelming predominance of sixth and seventh century finds. This precisely mirrors the commonness of grave-goods in Reihengräber of these two centuries; fifth century inhumations and burials from the eighth century onwards were largely without grave goods.

lack of finds implies the absence of *graves*, which we know continued to be dug in the late Roman extramural cemeteries and around other *memoriae* or churches built outside the town. The distribution map of finds reveals first and foremost cemeteries and secondly churches, but *not* occupation. This produces some interesting results at Trier. Schindler (1973) depicts the distribution of archaeological finds and known church sites on a map (fig. 2.2). The coincidence is almost exact outside the town walls, but inside there are many find spots without corresponding churches. Schindler tries to argue that it is wrong to see these intramural Merovingian finds as similarly deriving from graves (implying that this is the normal interpretation), but he does so with some strange claims for what can and what cannot survive from occupation sites. Yet the composition of these finds, brooches, belt-buckles, spearheads, and some pottery sherds, are precisely those common to burials. Surely the unassailable proof that they do derive from graves rather than occupation is that the *quantity* of finds from each chronological period mirrors the amount of grave-material generally found in the Trier region. Thus, graves of Böhner's period II yield few objects, but the number of finds from graves increases considerably for the remainder of the sixth and seventh centuries before drying up almost completely in the eighth. Exactly the same pattern is found in the scattered finds within Trier.

The lack of finds from ninth- and tenth-century Trier (likewise mirroring the paucity of known grave-material) is never taken as proof that Trier was completely abandoned in the late Carolingian period. Yet the absence of Merovingian finds is regularly posited as proof of abandonment of former Roman towns. If this argument were taken to its logical conclusion, we would find that the Franks had no settlements at all, for almost all the finds come from cemeteries. It is a well-known phenomenon that the Merovingians died in vast numbers, but appear never to have lived. Later, when it is time to reevaluate the evidence for urban settlement, I shall return to these stray finds that I assume were the product of burial rather than settlement. Such an assumption will allow somewhat different inferences to be made about occupation.

Now we may turn to an archaeological method for evaluating urban continuity that will surely repay future investigators handsomely: the study of the survival of Roman buildings and architecture.¹ Von Petrikovits (1958) concluded, in his article on the survival of Roman towns on the Rhine and Danube, that they did not. In an article recalling von Petrikovits' title, Schönberger (1973) begins by agreeing that they did not, 'in the strict sense of

1. I was unable to make use of the recent publication by Greenhalgh (1989) for the following.

town'. Schönberger effectively does no more than briefly summarise all the buildings known to have survived from Antiquity. The number is small. Thus for instance, Steuer (1980, 59) claims that at Köln, 'in only three places did the Franks probably take over Roman buildings within the town.' Even then, one of these, St. Maria, is considered to have taken over only the ruins of the capitol. Clearly, few will accept urban continuity if fifth- and sixth-century towns are viewed as a sea of ruins and rubble with only two or three Roman buildings left standing in the midst of such collapse. And Köln is well endowed with ancient relics, compared with other former Roman towns. Who would disagree with Reece (1980, 89) that 'a church and a palace do not make a town. Especially when they are set, admittedly inside a ring of ruined walls, amid fields of luscious green grass flourishing on our mysterious "Black Earth"'.? No one will argue that this hypothetical site is a 'town', whatever one understands by the term. But are these propositions of dilapidated post-Roman towns plausible?

When we look at the plan of the archaeologically recovered buildings in Burgundian Geneva – effectively little more than royal palace, cathedral, and other churches – should we hypothesise that the town walls enclosed nothing more than a few great buildings and luscious green grass (fig. 2.3)? We must ask if these visions of dilapidated post-Roman towns are plausible.

Firstly, we must recognise that our evidence of buildings that survived from the Roman period is extremely fragmentary. Schönberger (1973) does not give us a list of the Roman buildings of towns along the Rhine and Danube that survived Antiquity, but rather all those that have survived until today. He has given us a potted tourist's guide to sites like the Porta Nigra, Basilika, Barbarthermen, and Roman bridge in Trier. The list is that which has survived to *date*, either as a tourist attraction or as a well-known excavation; it is not all that survived the fourth century into the early Middle Ages. Steuer's two Roman buildings and a Frankish building on Roman ruins in Köln are only three that we know for certain; how many more do we know nothing about?

Secondly, we must try to understand the processes of decay better than we have done. From a technical, archaeological perspective the colourful pictures of decay and neglect offered by Steuer and Reece are, to my mind, in some ways impossible. Given our European climate the buildings of these nearly totally abandoned towns would soon have fallen down and been covered in vegetation and soil, Reece's luscious green grass and black earth, unlike the great preserved Roman ruins of Thugga, Tunisia. Soil formation should have covered the debris so that, where later building has not removed it, we should find a scene of utter dilapidation. But why then, when excavated, do towns never yield this veritable

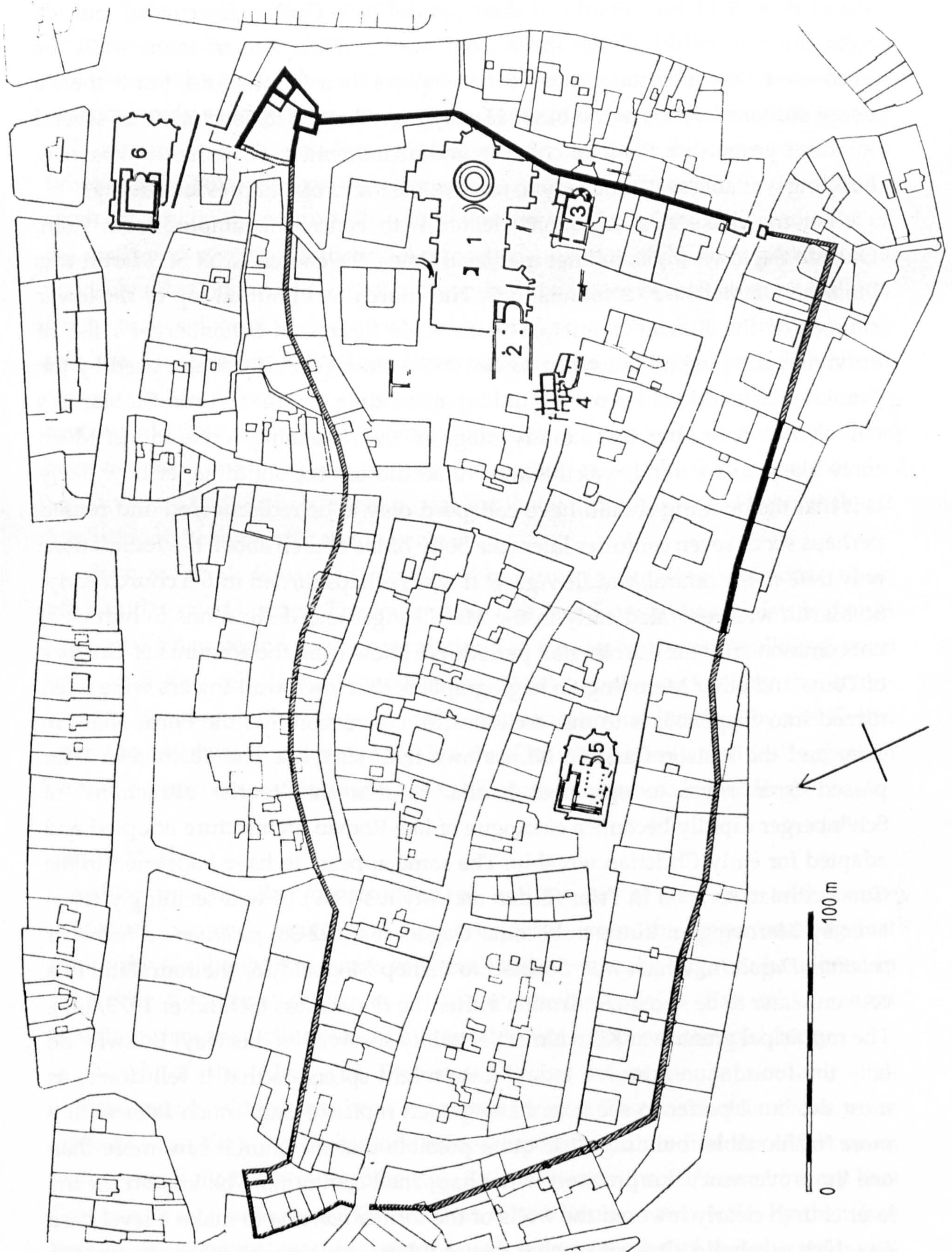


Fig. 2.3 Burgundian Geneva. Note that only the palace, cathedral, several churches, and town wall have been uncovered archaeologically (after Bonnet). Are we to believe that this was all the urban inhabitation there was?

sea of debris? Clearly Franks in their squalid little *Grubenhäuser* living outside Köln did not collect all the *spolia* from inside. When Roman town walls are uncovered, they may stand many courses above their foundations, but is there a dense scatter of debris at the base? Hardly ever. Are we to conclude that several hundred years after the total collapse and abandonment of Roman towns, new building was able systematically to remove the traces of that previous decay?

Let me make this argument clearer with concrete examples from Köln. Outside the town walls, in later medieval times, the church Groß St. Martin was built, now famous for its foundations. The church was built on top of the lower courses of the Roman municipal *horreum*. It figures in Schönberger's list of surviving antique buildings, but Steuer (1980) claims that it was not used by the Franks, who could not keep the building upstanding. In other words, St. Martin's church was only later to take advantage of the remaining foundations. Much more likely, to my mind, was that the Franks did use the building, for how likely is it that the building should have collapsed only to be rediscovered and reused perhaps six or seven centuries later (the body of the church above the foundations only date to the central Middle Ages)? It was perhaps turned into a church early; St. Martin was venerated early in the Middle Ages and dedications to him were not common after the Carolingian period. We know from the accounts of Gregory of Tours and other Merovingian hagiographers that town wall-towers were often turned into chapels. It is to this ecclesiastical conversion that the Porta Nigra in Trier and the Maison Carrée in Nîmes owe their survival. The Basilika in Trier passed from royal to episcopal hands, so that the 'tourist attractions' of Schönberger rapidly become a catalogue of late Roman architecture adopted and adapted for early Christian worship. The same appears to have happened to the Constantinian *horreum* in Trier (Eiden and Mylius 1949). It was seemingly taken over by Merovingian kings to become Dagobert's (622-38) *palatium ad horrea* or *palatium Dagoberti*, which was donated to Bishop Modoald for the foundation of a convent, later to be named St. Irmina's after the first abbess (Schindler 1973, 146). The municipal granary at Köln almost certainly survived in this way. But why do only the foundations survive today? One might speculate that it fell down, as most do, but I prefer to see it as having been replaced only much later with a more 'fashionable' building. It is quite possible that the church saw more than one 'improvement', but precisely what happened cannot now be known for the later church clearly lowered the walls of the former *horreum* to make a level base on which to rebuild. One must ask if this could not explain why other churches in Köln are built on Roman 'ruins': the form of the Roman buildings was not deemed suitable at some later date and the building was modernised.

In Köln it was not the careful excavation, but the careful analysis of the destruction of the *praetorium* that lead Doppelfeld (1970; 1973a; 1973b) to argue that the building must have stood until the Carolingian period. Steuer (1980) notes that only a single Merovingian coin comes from the *praetorium*. With that he calls into question its post-Roman existence. In precisely the same way Dodie Brooks (1986, 86) questions the continuous use of the *principia* in York until the ninth century on the grounds that 'very few artefacts were found . . . and none of the sixth and seventh centuries.' And yet, an absence of Merovingian finds is precisely what one would expect, unless there had been construction on the site, for the archaeological finds are material in the destruction debris or from pits dug into the site. It is unlikely there were pits cut into the reception hall while it served as a Merovingian or Anglo-Saxon king's palace! We might ask how many finds would now come out of the amphitheatre at Arles and of what date. If the answer were almost none of the last five centuries, would that prove that the amphitheatre had not been used in post-medieval times? Or would it prove that it has been used continually and kept clean?

Such survival demanded considerable effort in building maintenance, an often under-rated burden. The impression one gets from archaeologists is that in the post-Roman period nature was allowed to take its course and buildings were used until they fell down. Ward-Perkins (1984) has shown for Italy that maintenance of urban buildings was costly, but seriously undertaken by the Goths, later the Lombards, and naturally by the Church. A similar critical approach would increase our understanding of Gallic post-Roman urban buildings.

Town Walls

Consider now another construction of antique towns and one with better evidence than any *praetorium*. One can infer from the arguments against continuity (explicitly in the case of Reece) that town walls ought to have been in ruins like the rest of the town, with the possible proviso that unlike roofed buildings they are architecturally more stable and less prone to rapid decay.

North of the Alps and Pyrenees I know of only one site at which archaeological evidence has been suggested for post-Roman construction of town walls besides York. At Carcassone a local tradition claims that the oldest town walls were built by the Visigoths (fig. 2.4). Although the *Bordeaux Itinerary* lists Carcassone as a *castrum* in 333, and thus it had Roman town walls by then (see later in this chapter), local historians have clung to the belief that the walls still standing were built later. Yves Braund prefers a Roman date but is willing to entertain a date around 500 to fit the political situation and to explain why the

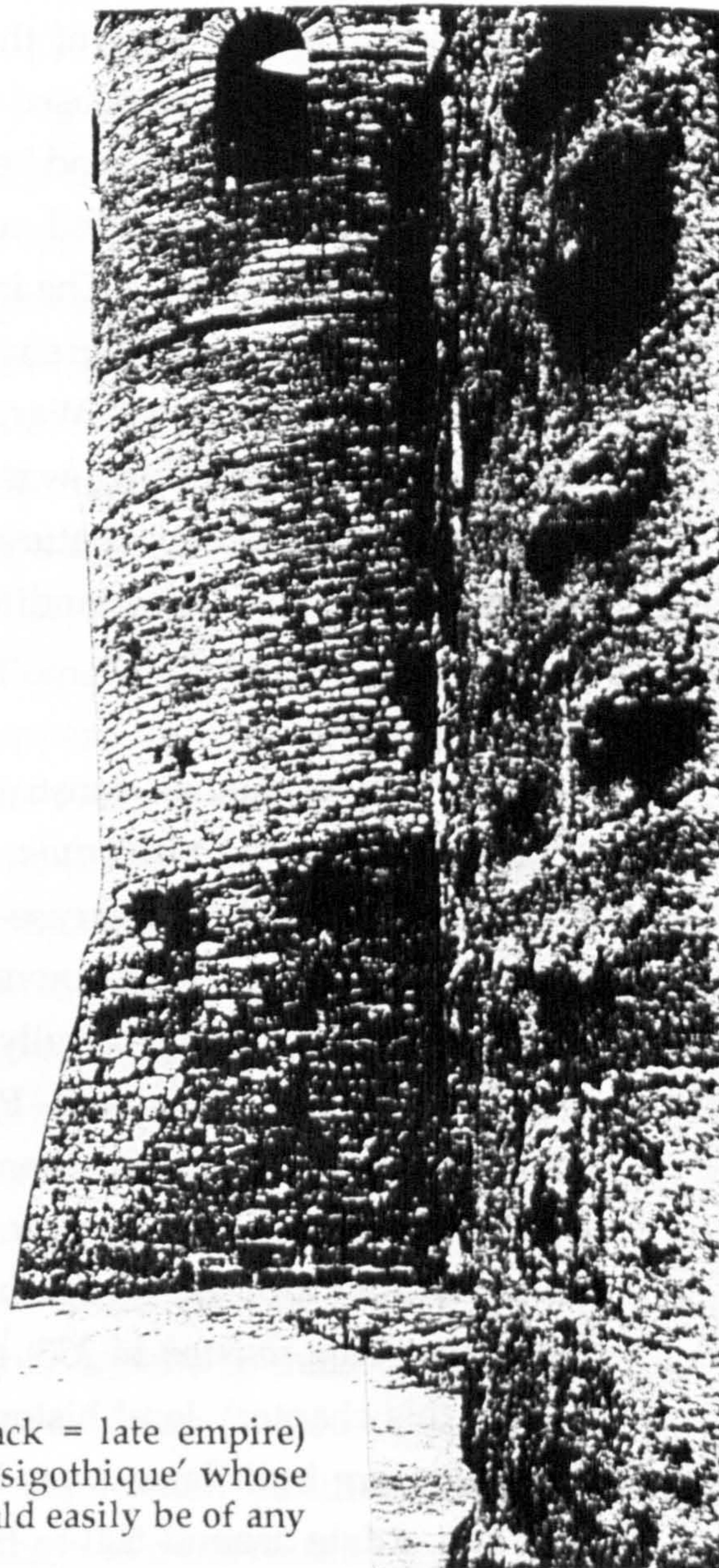
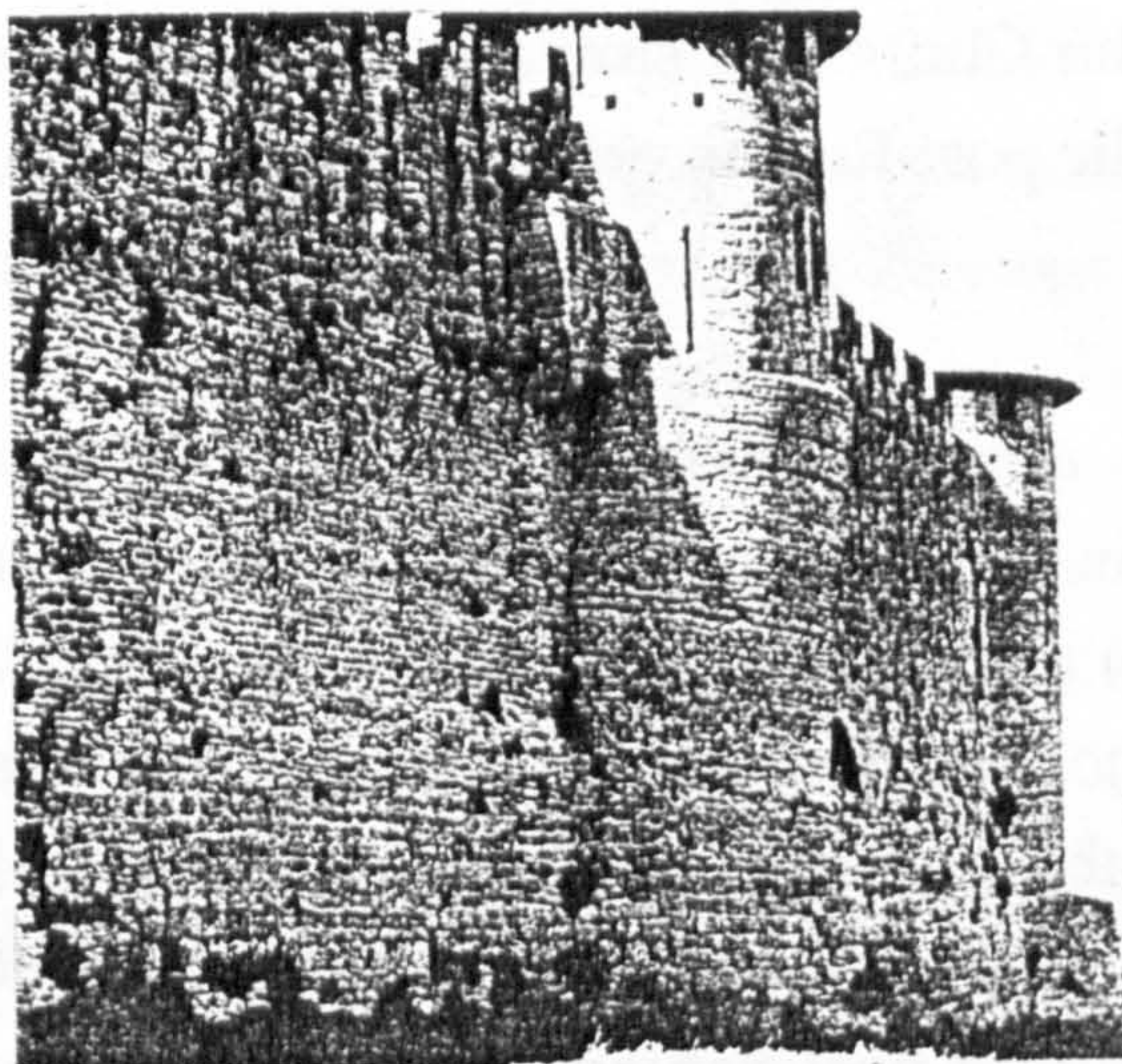
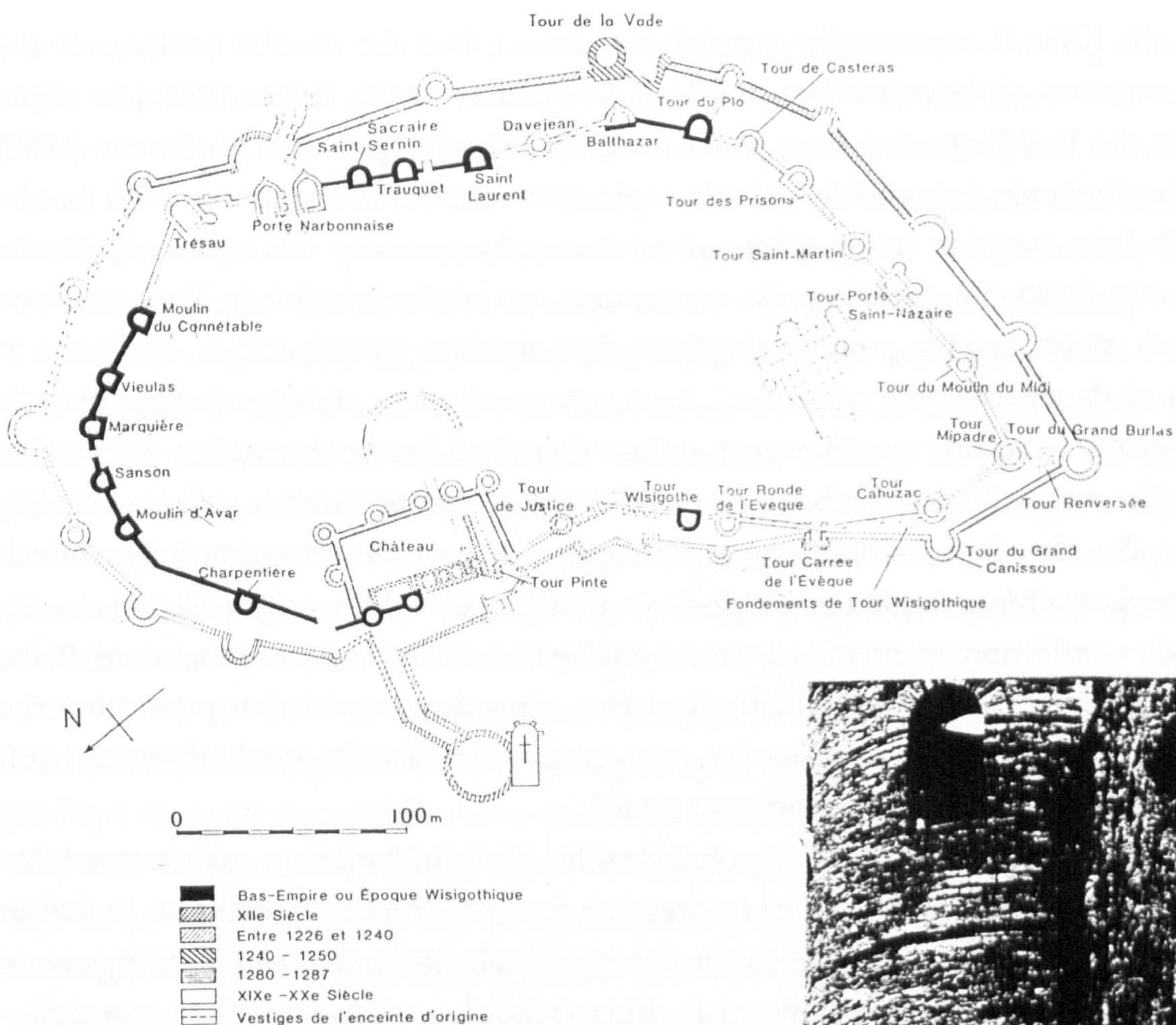


Fig. 2.4 Carcassonne. Plan of town walls (black = late empire) (after Cros-Mayrevieille); photo of the 'tour wisigothique' whose battered base is supposedly Visigothic, but could easily be of any date; and other Roman towers.

circuit is not rectangular, but a quick glance at comparative plans of late Roman town walls reveals no such regularity. To no one but a local historian or archaeologist is there any doubt that the walls are Roman.

Elsewhere the walls do not survive to a sufficient height to tell us more. The only clue can come from the post-Roman layers around the base, which can tell us a) nothing, b) that the wall collapsed, or – making up the majority of instances – c) that if it did collapse, the stone was collected and taken away. Gregory of Tours describes Dijon as it appeared in the late sixth century (HF 2.23):

It [Dijon] is a *castrum* girded round with mighty walls and set in the centre of a pleasant plain. Its lands are fertile and so productive that, after a single ploughing, when the fields are sown, a rich harvest soon follows. On its southern side it has the River Ouche, which teems with fish. A smaller stream runs down from the north, entering through one gateway, running under a bridge and then flowing out again through another gate. This stream washes all the fortifications with its gentle waters and turns the mill-wheels round at wondrous speed outside the gate. The four entrances to the town are placed at the four quarters of the compass, and thirty-three towers adorn the circuit of the walls, which are made of squared stones rising to a height of twenty feet, with smaller stones placed above to reach in all some thirty feet, the whole being fifteen feet thick. Why Dijon is not elevated to the dignity of a *civitas* [bishopric], I cannot imagine.

Archaeologically, Gregory has been proved correct in his description of wall width, its lower courses and the number of towers. His description of the different sized stones of the upper courses are quite in keeping with other examples of Roman architecture. Archaeology, however, cannot prove what the upper courses looked like at Dijon (or almost any other Roman town) or whether they were Roman or Merovingian.

Indicative of just how little post-Roman evidence there is can be gleaned from the plans of twenty-one French cities in Carlrichard Brühl's study (1975). Each locate the wall circuits of different ages, from the first to the nineteenth century AD, in various colours. Violet marks those wall circuits built during the sixth to ninth centuries and is found only on the plan of Vienne, and this stretch Brühl suggests was built after 882 AD. In short, Merovingian town walls were Roman town walls.

Accepting this premise one could produce a list of walled towns in Merovingian Gaul based on the archaeological evidence assembled by Stephen Johnson (1983). To it could be added cities for which there is evidence from early mediaeval documents. A thorough search would probably add almost every other *civitas* from the *Notitia Galliarum* not already on Johnson's list. It is presumably this very documentary evidence which allows Edward James (1982, 46) to state confidently that in Merovingian Gaul 'the turreted and battlemented walls which had been constructed around almost all Gallic towns in the third and

fourth centuries, survived'. It is a view shared by almost all (e.g Fournier 1966, 36-7; Salin 1950-9, 431). That walls were generally complete is suggested by Gregory of Tours, who systematically recorded their collapse as noteworthy portents, and thus presumably an infrequent occurrence. The walls of Angoulême were said to have fallen at the mere approach of Clovis (HF 2.37). Parts of the city walls of Lyon were undermined when the confluence of the Rhône and Saône overflowed its banks, the walls of Bordeaux were in danger of collapse after an earthquake, and those of Soissons collapsed in 582. All were described amidst tales of freak weather, blood pouring from loaves, fire from heaven, lights in the sky, an eclipse, wolves invading the cities, and plague (HF 5.35, 6.21). More commonly the walls appear in perfect condition in the writings of Gregory. Theudulf a deacon from Paris died falling off the walls of Angers when very drunk after visiting the bishop's *solarium* built high on the city walls (HF 10.14). The Thuringian king, Hermanfrid, was 'accidentally pushed' from the walls of Zülpich, while the chanting of psalms by inhabitants walking around the top of the walls saved Bazas, Clermont, and Saragossa during sieges (HF III.8, GM 12, VP 4.2, HF 3.29). Information from Gregory allows us to infer further that one could walk around the circuit of town walls at Arles, Avignon, St-Bertrand-de-Comminges, Vienne, and Tours (HF 4.30; 6.26; 7.36; 2.33; VM 1.23). A homily (24) of Avitus tells us that Lyon was more protected by its basilicas than its bastions – by God rather than the arms of men – imagery that was unworkable if the walls were ruined. The anonymous *Song of the Watchmen of Modena* was sung by Carolingian priests probably in a chapel of the city walls – Roman walls – at Modena (Godman 1985, 324-7):

O you who guard those walls with arms,
do not sleep, I warn you, keep watch!

There is also evidence that dilapidation was in fact set right. We know that King Chilperic was anxious that, in the face of invasion by his brother and nephew, the walls of his cities were in good repair and ordered his dukes and counts to see that they were (HF 6.41). From Geneva comes a stone bearing an inscription that would have read in full: *Gundobadus rex clementissimus emolumentio proprio spatio multiplicato* (Martin 1975). King Gundobad apparently used his own funds to rebuild or to enlarge part of Geneva's town wall. Other sources mention the repair of the walls at Viviers by Saint Venance and Cahors by Saint Didier. The *Vita Desiderii*, singular in its seeming historic accuracy, provides copious evidence of building works undertaken by St. Didier at Cahors after his installation in 630 (Rey 1953). His works included a *domus ecclesiae*, innumerable churches, a monastery, bridges, and it would seem the town walls, towers and

gates (*ecclesias, domos, portas, turres murorum ambitu ac quadratum lapidum compactione munuit . . .*). Although Johnson's (1983) distribution map lists Cahors as not yielding any evidence of Roman walls, Rey has reproduced the supposed course of Roman ramparts (presumably from no more than street plans and a good deal of faith) and those of St. Didier and the probable location of the churches he founded (fig. 2.5). Too much could easily be made of this hypothetical line of a Merovingian town wall, although some credibility is gained if St. Amans is rightly located, for the *Vita* does claim it to be extramural.

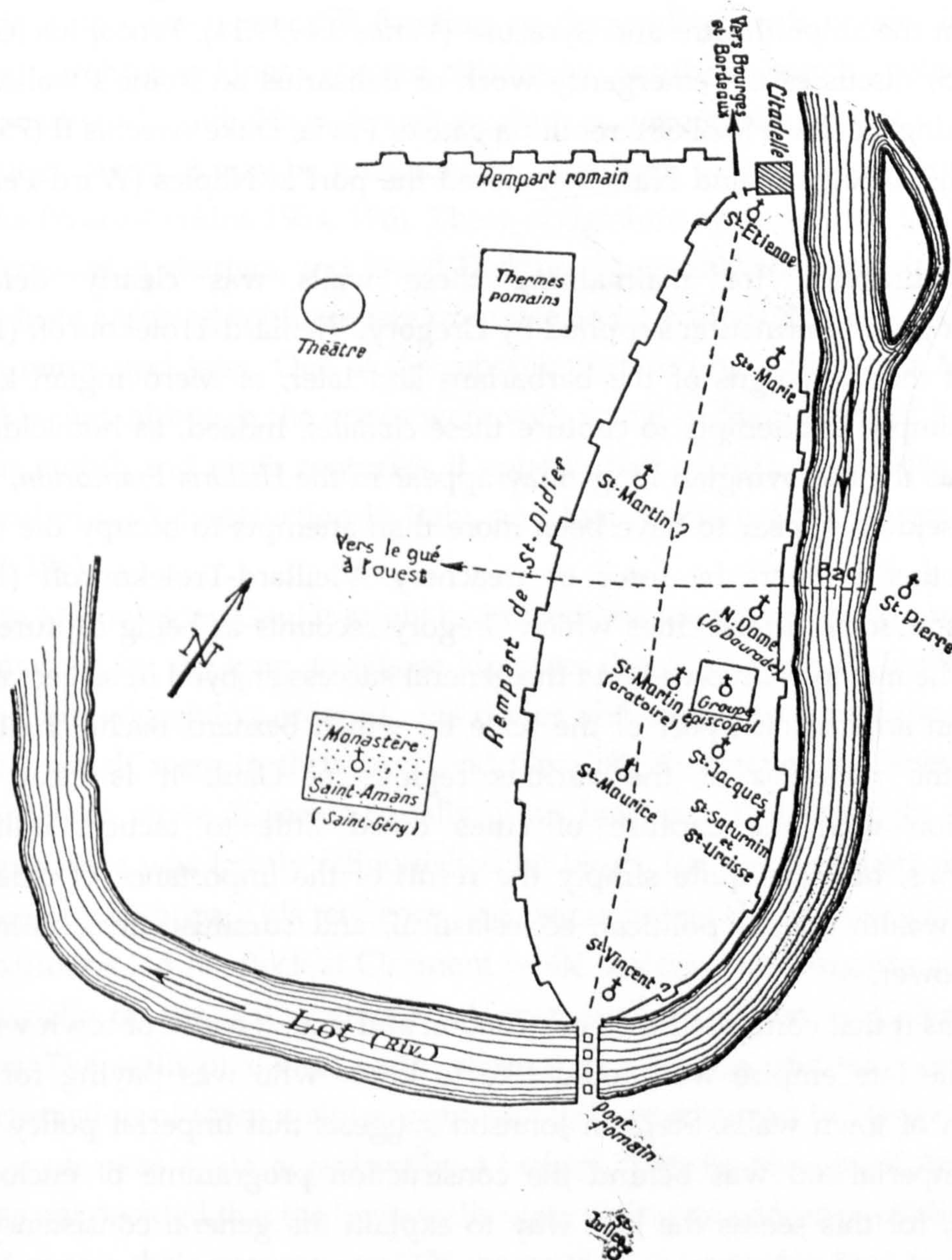


Fig. 2.5 Cahors. Note the rampart (situation hypothetical) built by Saint Desiderius, whose Life records that the monastery of St-Amans was extra muros (after Rey).

If one looks to Italy for parallels, one finds much more evidence of upkeep, although even more conclusive evidence for the lack of large-scale new construction in the early Middle Ages. At least three *castra* appear to have been created *de novo* under Theodoric as evidenced in his letters written by Cassiodorus (*Variae* 1.17, 3.48, 5.49), while the only really major undertaking was the ninth-century Leonine wall at the Vatican in Rome. According to the Anonymous Valesianus, Theodoric the Great built new walls around Verona and Pavia, although it is much more likely that these were only restorations (Ward-Perkins 1984, 192), and Theodoric's letters do reveal refurbishing at Catina with stones from the amphitheatre and Syracuse (*Variae* 3.49, 9.14). Procopius (*Gothic War* 1.14.15) discusses the emergency work of Belisarius on Rome's walls. The Lombard king, Perctarit (672-88), rebuilt a gate of Pavia; Duke Arachis II (758-87) added walls to Salerno; and Narses enclosed the port at Naples (Ward-Perkins 1984).

The justification for maintaining these walls was clearly defence. Summarising the information supplied by Gregory, Vieillard-Troiekouroff (1978) claims that the campaigns of the barbarians and later, of Merovingian kings, consisted simply of attempts to capture these *civitates*. Indeed, as homicidal or fratricidal as the Merovingian kings may appear in the *Historia Francorum*, their civil wars seldom appear to have been more than attempts to occupy the cities ruled by other brothers by force or treachery. Vieillard-Troiekouroff (1978) reminds us of some fifteen cities which Gregory recounts as being captured or besieged. The methods involved and the general success enjoyed in siegecraft by Merovingian armies form part of the scale by which Bernard Bachrach (1972) measures the *romanitas* of the various regions of Gaul. It is clear that preoccupation with the capture of cities owed little to tactical military considerations, but was quite simply the result of the importance of cities as sources of wealth and as political, ecclesiastical, and administrative centres – centres of power.

Who was it that controlled the construction and maintenance of town walls? Even for the late empire we should like to know who was paying for the construction of town walls. Stephen Johnson suggests that imperial policy and probably imperial aid was behind the construction programme of enclosing major cities, for this seems the best way to explain the general consistency in construction techniques. The smaller centres, which differ markedly from neighbouring large cities, for example Noyon amid its *Belgica secunda* neighbours, were perhaps left to provide their own defences. The numerous *vici* in Gaul that faced the fifth century without fortifications perhaps did so because

the cost of such a 'fantastically expensive operation' deterred the local dignitaries (Johnson 1983, 116). The need for imperial sanction for the building of town walls is thought by Johnson to have been a formality, by the third century, and certainly not a reason for the absence of walls.

This arrangement appears to have continued in early medieval Italy. Theodoric's letters, as we have seen, show the active role of the king in town-wall building. *Variae* 3.44 reveals Theodoric contributing funds to the restoration of the walls at Arles. Another letter (1.28) exhorts Goths and Romans alike to give suitable stones found in their fields to those engaged in rebuilding town walls. Public authorities were still funding or demanding wall repairs in Lombard times. Carolingian kings expected citizens to contribute church-, palace-, bridge-, and road-work, and although wall-work does not actually appear in capitularies as duties owed, it may be presumed to have been part of these duties of public works (Ward-Perkins 1984, 196). These obligations were termed 'ancient duties' in Carolingian charters, and Ward-Perkins argues that they were more likely to have been enforced continuously from late antique times than to have lapsed and been reimposed later. One of the most important implications for Merovingian Gaul is that, although the popes were very active in the upkeep of Rome's walls in the eighth and ninth centuries, it appears that secular authorities maintained control of wall construction in Italy, and that it was not taken over by bishops (ibid, 196).

In Merovingian Gaul it might be immediately assumed that control would be maintained by the king, to whose kingdom the city belonged. Indeed, we have seen Chilperic giving orders to his dukes and counts to repair the walls of his towns which were in their trust and Gundobad's repairs at Geneva, his *sedes*, recorded in stone. It seems equally clear, however, that the right and duty of maintenance was lightly relinquished by kings, for we hear most often of their upkeep by bishops. This tendency had begun before the Merovingians; Sidonius Apollinaris led his flock at Clermont while besieged by Visigoths and appeared personally on the walls, while St. Aignan similarly defended Orleans from the Huns. The walls of Viviers were repaired by a bishop, and the most impressive reorganisation of town walling supposed to have occurred in Merovingian times, at Cahors, was again organised by a bishop. Had the bishops of Angers and Le Mans not decided that the town walls were their own concern, building a *solarium* on them for their personal use? Of course there is undoubtedly a bias introduced by the sources which are predominantly hagiographic, so that bishops are no doubt over-represented. It seems likely that king, duke, or bishop, whichever the locally pre-eminent, felt it his right and duty to make any decisions pertaining to

the town walls. These local guardians seem to have taken a relaxed view of their responsibilities, exemplified by the creation of a *solarium* on the walls of Angers and Le Mans, and by the seeming sluggishness of ducal action in maintenance suggested by Chilperic's order to ensure the siege-worthiness of his towns' walls. To explain the reticence, we must look at the more zealous repairers, namely the bishops. Undertaken at their own expense, their actions are praised as benefiting the whole community, for town walls offered communal protection, not simply protection to the favoured few. The resultant expense must surely have brought prestige; Desideratus certainly was held in high regard by his community. The social and financial value of such prestige was probably often important within the *civitas*. Whenever the sources allow a closer look at local politics, as Gregory's works do for Tours and the *Lives of the Fathers of Mérida* does for Mérida in Visigothic Spain, we see political or familial groups with sharply conflicting interests. We might speculate that emphasis on community spirit through such acts as maintaining city walls was more likely when there was competition for the position of municipal pre-eminence, perhaps between bishop and count. However, overtures of public spiritedness could not compete with more tangible rewards to would-be supporters, and rivalry was more violent than the competition for municipal office in the Roman period. It is quite likely that public munificence was less important to Merovingian politics; one did not court favour with voters, but used rather more direct methods of maintaining one's partizans and detaching others from their leaders.

Probably more common than the generous political gift was the extraction of money or labour from the *cives* by the local authority to maintain the walls. Resistance to taxation in Merovingian Gaul was undeniably current and the lack of enthusiasm to spend public money once in the coffers was a common allegation aimed at Merovingian kings, dukes, and counts. If collection of taxes became more difficult throughout the Merovingian period, it is not difficult to understand the general lack of interest in maintenance that we see in Carolingian times.

This discussion of town walls has introduced the other major source of evidence used to reconstruct early medieval urban topography, namely written sources. Many plans might be presented here to exemplify what is known of early medieval town lay-out from documents. We have already seen Trier. To this let us now add Tours and Le Mans (fig. 2.6). One will immediately notice how well informed we are about the churches. The reason is simply that our documentary

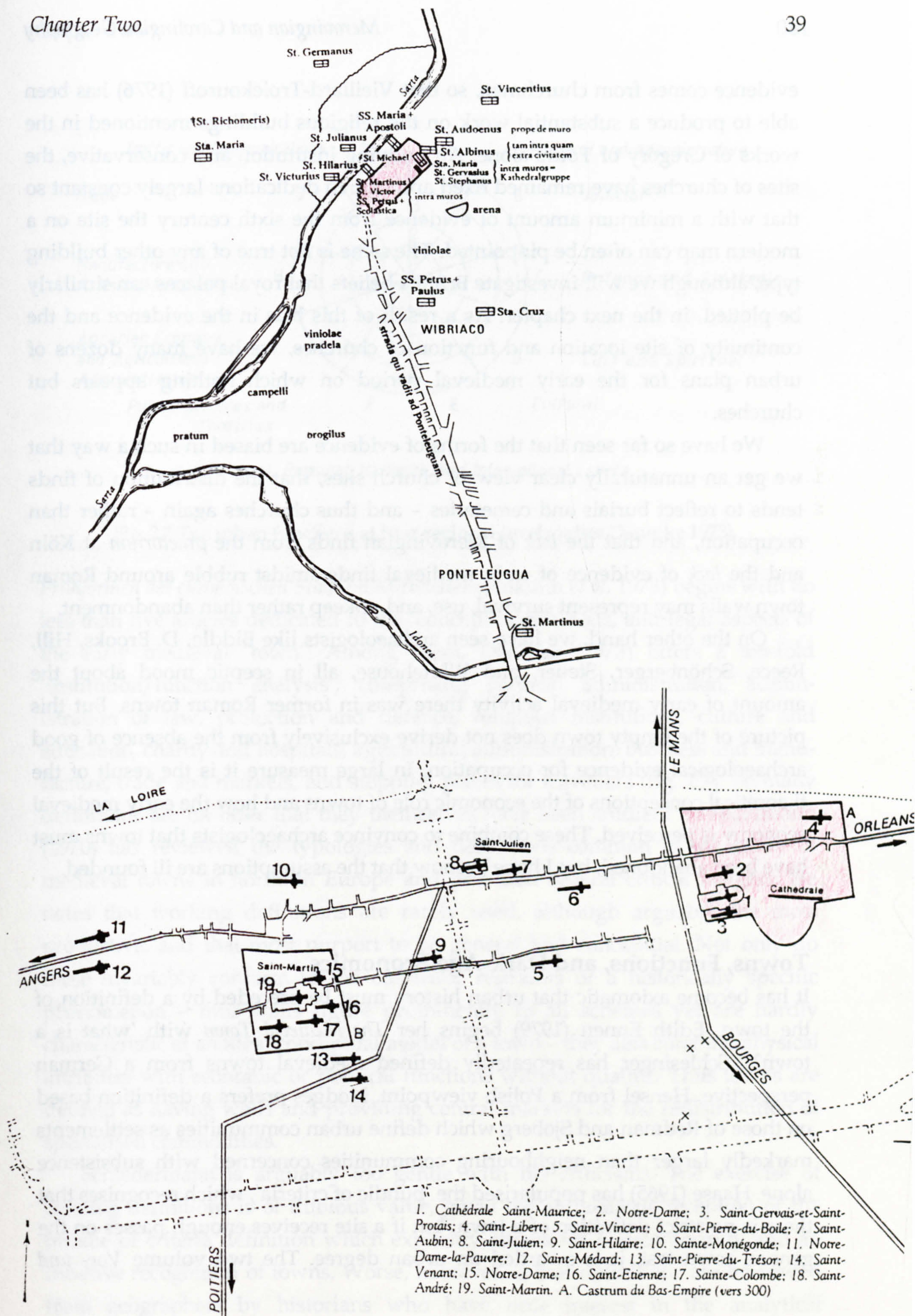


Fig. 2.6 (facing) Tours. Roman town walls and churches of the seventh to ninth century (after Galinié). Le Mans, ca. 700: churches and Roman town

evidence comes from churchmen, so that Vieillard-Troiekouroff (1976) has been able to produce a substantial work on the religious buildings mentioned in the works of Gregory of Tours alone. An undying institution and conservative, the sites of churches have remained fixed and church dedications largely constant so that with a minimum amount of evidence from the sixth century the site on a modern map can often be pinpointed. The same is not true of any other building type, although we will investigate Brühl's beliefs that royal palaces can similarly be plotted, in the next chapter. As a result of this bias in the evidence and the continuity of site location and function of churches, we have many dozens of urban plans for the early medieval period on which nothing appears but churches.

We have so far seen that the forms of evidence are biased in such a way that we get an unnaturally clear view of church sites, that the distribution of finds tends to reflect burials and cemeteries – and thus churches again – rather than occupation, and that the *lack* of Merovingian finds from the *praetorium* at Köln and the *lack* of evidence of early medieval finds amidst rubble around Roman town walls may represent survival, use, and upkeep rather than abandonment.

On the other hand, we have seen archaeologists like Biddle, D. Brooks, Hill, Reece, Schönberger, Steuer, and Whitehouse, all in sceptic mood about the amount of early medieval activity there was in former Roman towns. But this picture of the empty town does not derive exclusively from the absence of good archaeological evidence for occupation. In large measure it is the result of the theoretical conceptions of the economic role of towns and how the early medieval economy is perceived. These combine to convince archaeologists that towns must have been abandoned, but I hope to show that the assumptions are ill founded.

Towns, Functions, and Dark-Age Economics

It has become axiomatic that urban history must be preceded by a definition of the town. Edith Ennen (1979) begins her *The Medieval Town* with 'what is a town?'. Schlesinger has repeatedly defined medieval towns from a German perspective, Hensel from a Polish viewpoint. Hodges prefers a definition based on those of Redman and Sjöberg which define urban communities as settlements markedly larger than neighbouring communities concerned with subsistence alone. Haase (1965) has popularised the 'bundle of criteria', which recognises that there is no strict definition of a town, but if a site receives enough passes on the general town test, it is awarded an urban degree. The two volume *Vor- und*

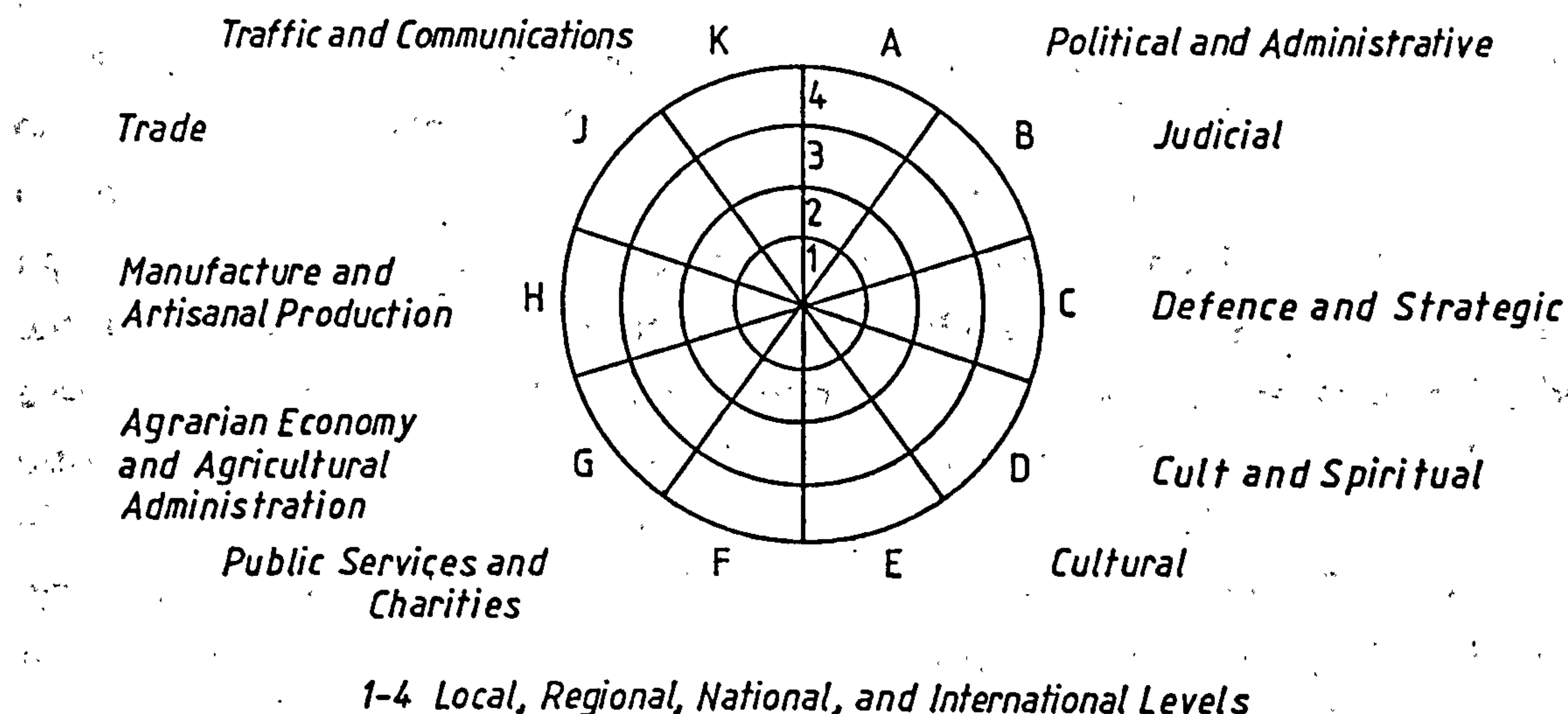


Fig. 2.7 Ten urban functions at four regional levels (after Denecke 1973).

Frühformen der europäischen Stadt im Mittelalter (Jankuhn *et al.* 1973) begins with no less than five articles dedicated to the 'concept', definitions, and legal aspects of the early medieval 'town'. Among them, Denecke (1973) offers a ten-fold 'institution/function analysis', comprising political administration, administration of law, protection and defence, religious institutions, culture and education, charity and hospitals, agricultural administration, business and manufacture, trade and markets, and stopping places for travellers (fig. 2.7). So many definitions are on offer that they themselves have been studied! Schledermann (1970) has reviewed the typologies and definitions common in the study of medieval towns in northern Europe and provided several critical insights. He notes that working definitions are rarely used, although arguably the most productive, and that most purport to be general and conceptual. Not only do these invariably conform to the empirical restraints of a historically specific phenomenon – thus walls figure prominently in all schemes yet are hardly characteristic of an ideal conceptual model of a town – they also combine physical attributes with economic or political functions without qualms. Thus towns are defined as having walls and providing central markets for the redistribution of specialised commodities.

Schledermann is arguably too gentle with his criticisms. The exercise of creating definitions is of dubious value, given the enthusiastic reception of the bundle of criteria definition which explicitly allows for non-conformity and an intuitive recognition of towns. Worse, however, is that definitions are borrowed from geographers by historians who have little interest in the analytical

framework in which the definitions are produced. Thus the central-place functions are drawn from a modern capitalist market economy, and cities of a modern world-wide capitalist economic system form the basis for the definitions that historians use of medieval towns. Ennen (1979, 20) points to economists who discuss the medieval town with its market as 'an "invention" which helped to solve the early problem of co-ordinating division of labour in the economy and thus it made possible far-reaching specialisation by the producers in the secondary and tertiary sectors.' Ennen does not accept that there is no more to a town, but not because it is a model for capitalist economies, but rather because they also provided the seat of political, judicial, and administrative authority. The result is that towns are seen as centres of manufacture, services industry, administration, and markets of product exchange and they therefore appear even more like modern urban centres.

In essence, Ennen provides only a more generalised picture of urban 'functions' than that of Dennecke. The idea of 'providing' is, however, very widespread. When von Petrikovits (1958) sought to evaluate the amount of continuity of Roman towns on the Rhine and Danube he used an analytical approach which questioned the survival of religious, administrative, trade, manufacture, and defensive 'functions'. Towns would seem to have much to offer the countryside. To most historians, on offer is a whole range of urban 'functions'. Among others one can see, if only indirectly, the belief that there must be an economic base on which administration and services - the superstructure of urban 'functions' - were built. This base is almost universally seen as manufacturing and trade. To my mind, this is to seriously misunderstand the nature of early medieval and even Roman towns.

This functional approach, at its worst, discusses the provision of religious services, ceremony, and belief but without much regard for the political and economic aspects of religion. Church estates cannot quite find a place in the discussion because it is couched in terms of functional services provided to urban dwellers and town visitors. At its best, the functional approach takes up the economic mantle as the essence of town life. Further analysis may then centre on the position of towns within economic structures. Here Pirenne continues to cast his long shadow.

Pirenne's belief, encapsulated in *Mahomet et Charlemagne*, was that the Merovingian and Visigothic ages were effectively a continuation of late classical Antiquity, albeit in a less glorious form, and that only the closing of the Mediterranean basin by Islamic conquerors finally plunged western Europe into the Dark Ages. Today the Pirenne thesis is generally held to be incorrect,

although it still forms the structural framework on which to hang new ideas and around which to organise lecture courses. Nothing illustrates this more clearly than the fact that Hodges and Whitehouse explicitly organise their book, *Mohammed, Charlemagne & the Origins...*, around the Pirenne thesis, although they do not agree with it.

So what is the nature of the broad general disagreement with Pirenne's thesis today? Basically it is one of date. Pirenne put forward the unorthodox thesis that the fifth century did not represent a major break in western civilisation, although it had long been seen as the end of civilisation. More importantly, he argued that the fifth century did not witness a major break in western trade. Those who disagree with Pirenne simply want to put the great collapse of the world as the Romans knew it back to the fifth century once again.

There is, however, widespread agreement with Pirenne in essence. So essential are the shared theoretical premises, many scholars may have overlooked them, for they were taken for granted. This essence is the role of trade, of its paramount importance in the economy.

Pirenne used written sources to show the continuity of Mediterranean trade. Syrian merchants, papyrus, spices, and silks all appeared in texts, be they histories, saints' lives, or official grants of freedom from tolls. Archaeologists today use excavated objects to show the decline in trade of Red African slip wares and comparable pottery from further eastern shores of the Mediterranean after the fifth century and the virtual cessation by the mid-sixth century. They document the collapse of marble quarries and export ports like Luna. With numismatists they reveal a decline in gold content in coins, the disappearance of silver and copper coinage, and a marked decrease in the amount of specie. Without money how could there be markets, how could there be trade?

So it is that the shadow of Pirenne and his opponents have debated markets, money, and trade. This has become synonymous with economic activity, and this economic activity has become enshrined as the necessary essential for the existence of 'real' towns. Unlike the bureaucratic trimmings of royal or ecclesiastical ceremony the provision of markets and the opportunity to trade are seen by these scholars as the real purpose of towns. Therefore those who oppose Pirenne are led to the conclusion that economic conditions for towns did not exist in western Europe in the fifth to eighth centuries: no trade in pottery or marble, no proper coinage, no towns. Where I disagree with both camps, pro- and anti-Pirenne, is that I hold that the majority of economic activity in Antiquity and the early Middle Ages happened without money and without markets, that trade only played a minor role in the totality of economic activity, and that towns were

not the product of trade or markets nor were they greatly dependent on them.

A prime example of the anti-Pirenne camp, which nevertheless shares the belief that trade was essential to economic well-being and the existence of towns, is Richard Hodges. His work, *Dark Age Economics*, is subtitled 'the origins of towns and trade AD 600-1000', but is almost exclusively concerned with *emporia*, the coastal trading settlements of the eighth to tenth century, including Hamwih, Ipswich, Dorestad, Hedeby, Birka, and Helgo. But where is Marseille? Pirenne would have wept at the thought of early medieval towns and trade discussed without reference to towns of Roman origin. Hodges does mention them briefly to dismiss them, suggesting that Tours and Trier could be compared with Clonmacnoise and Kildare, sites of many churches but little urban housing or industrial and artisanal production. The period 500-800, in Hodges' view, could not have supported urban centres because commercial market places hardly existed. Fifth and sixth century England 'was a world without either consumer cities or peasant markets' (Hodges 1988, 3). Because Dark-Age economics, trade, and urbanism have all become more or less synonymous we find that Hodges concentrates on *emporia* where evidence of long-distance trade is found and that to him these are the only real towns engaged in real economic activities. Many archaeologists have followed suit and these *emporia* have been raised to the status of the most important urban agglomerations in early medieval western Europe. Verwers (1988, 55) notes that in an article published in *Scientific American* 'Dorestad was called the greatest town west of Constantinople'!

Where Hodges differs from most medieval scholars is that he interprets *emporia* as the product of politically controlled trade, which preceded true mercantile activity. His arguments derive from economic theories based on anthropological work on exchange within precapitalist societies. I believe there is much to be gained from this body of thought. Indeed economic anthropology probably could explain the nature of some of the trade, like that of the Vikings or the Frisians. However, Hodges has applied models from rather less developed, more egalitarian societies and made Charlemagne and Offa into quasi-bigmen of New Guinea. There is little in Hodges's work to suggest the importance of dependent agents sent out by great monasteries or regional potentates. These did not rely on the control of exotica or lava quern-stones to maintain their power, which was measured in hundreds of dependent farming households. A more incisive use of such economic anthropology is presented by Whittaker (1983).

Another problem with stressing the trading preeminence of *emporia* is that we cannot say that the former Roman towns did not have comparable or even greater trading connections. Because continuous occupation has eradicated much of the

early medieval record, and because most continue to be heavily built-up and therefore large-scale excavation is not possible, these former Roman towns cannot hope to yield evidence in the manner of Hedeby or Dorestad. But now that evidence is coming from London and York of just such long-distance trade, it is odd that they are having the epithet *emporia* appended to them. Although we cannot see it clearly, glimpsing it only through the archaeology at Billingsgate and the Merovingian textual evidence gathered by Pirenne, it seems certain that London and Marseille, the great Roman port-cities, remained the most important centres of trade in Dark-Age England and Gaul.

Returning to the equation of trade and manufacture with urbanism, we find that many archaeologists believe it only natural that early medieval towns could not have existed and the former Roman towns must have stood empty. Biddle (1976, 103) suggests that it was a great intellectual move forward to recognise that 'towns could revert to non-urban settlements before *re-emerging* as urban places in later centuries' (my emphasis). What made these towns 'non-urban' in Biddle's eyes was the 'collapse of the economy'. He (p. 110) depicts one town in this way:

There may have been little within the walls of Canterbury except a royal residence during much of the fifth and sixth centuries, but such a residence would express the underlying reason for the continued importance of the site – the exercise of an acquired authority from its traditional centre.

He extends such a view to Winchester and other Roman towns: empty but for a king with his hall and a bishop with his cathedral. We can see now why Biddle (1976, 116) once suggested that London could never have approached the size of contemporary Dorestad. Never, because Biddle was contrasting it with the supposedly real urban function of trade which he believes characterised *emporia*. He contrasts Winchester with Southampton: an old empty political centre with a new urban trade and manufacturing centre; a *civitas* with a *pagus*; a *caestre* with a *tun*. Chris Arnold (1984), with his systems collapse theory, draws even more heavily on the idea of a productive economic base of trade and manufacture, with a whole constellation of capitalist economic assumptions which are arguably totally anachronistic (Driscoll 1987).

Biddle has fused the economic function with some of the other functions towns are meant to provide: royal administration and ecclesiastical services. Of these the religious aspect of towns in Gaul has long been stressed. Colville (1928, 550-1) claimed the Lyon must have been primarily an 'ecclesiastical city'. French researchers have long spoken of the *ville sainte mérovingienne*. Jean Hubert (1959) went so far as to characterise the second of three stages of town evolution from the Roman period to the Middle Ages in such terms. To Latouche (1961, 98):

Thanks to certain attractions, most of them religious – one might even say superstitious – as for instance the presence of deeply venerated relics, Roman cities threatened with decay lingered on until such time as the revival of economic activity gave them a new lease of life; the flame had never been completely extinguished.

In short, the great Roman towns are seen to have been maintained by Easter services, occasional baptism, 'superstitious' genuflection *ad sanctos*, Christmas feasts, and occasional rituals of kingly magnificence: just enough services to keep the towns from disappearing completely. Von Petrikovits (1958) comes close to the same conclusion but then notes that relics were sufficiently 'mobile' that, if there were not other reasons for the population to stay, the relics would have gone with them.

Such an emphasis on the holy aspects of Merovingian towns is certainly the result of the importance of ecclesiastical presence. In the seventh century Vienne sheltered at least eleven churches and approximately fifteen monasteries (Pelletier 1974). In the time of Bishop Chrodegang (742-66) 38 churches are known in Metz from a quadregesimal station list (Stahl 1982). We have seen the graphic examples of urban topography dominated by churches at the start of the chapter. There I suggested that we are also misled by the evidence: churches mentioned in texts can usually be pinpointed if the dedication has not changed for the site of the church building seldom moved; the texts are predominantly ecclesiastical in production and interest. In short, if our plans of early medieval towns appear to consist of nothing but churches we must recognise that it is first and foremost the product of our evidence.

The empty Dark-Age town is seen by scholars to fill up again when widespread trade activity began to flourish. *The Rebirth of Towns in the West, AD 700-1050*, a recent CBA research report, dates it neatly. Ennen (1979) entitles the ninth and tenth centuries as 'new beginnings'. Schlesinger (1954) likewise places the ninth and tenth centuries central to urban growth for it is then that the spatial division of town centre and mercantile *suburbia* first appears – in documents at any rate. Again, the *Histoire de la France urbaine* series (Duby 1975) divides the first two volumes at the ninth century. This last section of volume one opens with the words: 'In the course of the seventh and eighth centuries, the last elements of Roman municipal organisation slowly disappeared.' Post-Roman urban history is apparently one of steady decline until the 'new beginnings' of the late- and post-Carolingian era.

Now there is no doubt that the ninth and tenth century saw changes in the economic and social conditions of western and southern Europe. There seems little doubt that trade did increase in importance, that the economy became widely monetised, that towns grew in size, and that urban growth was closely

connected with markets. It is, however, contentious to argue that the general absence of commercial mercantile activity in the fifth to ninth centuries presupposes the lack of urban centres. This is what I hope to explain, but first there is the empirical evidence to suggest that the Merovingian town was not quite as empty as it has been too often believed.

The Populous Town

In contrast to most early medieval archaeologists and some historians, there are traditional historians who suggest that, when Bede called London an emporium of a multitude of peoples who came by land and water (*multorum emporium populorum terra marique venientium*) he meant early seventh century London was very large, just as Roman London had been 'one of the largest towns in the western Empire' (Wacher 1974, 18). The same historians accept that Gregory of Tours knew what he meant when he talked of the throngs of people and the babble of different tongues in Orléans (HF VIII.1). There were *minori populo* of Comminges who were ejected before a siege (HF 7.34). This was apparently a common tactic, for the 'common' folk were similarly driven out of Vienne due to a food shortage when it was besieged (HF 2.33).

Then there are the descriptions of buildings themselves within towns, which seem to imply density of occupation. There are the crowded quarters of towns that regularly burned while the saint's church was miraculously spared in towns across all Gaul. Do these references not imply a greater population than the king and bishop alone rattling around the deserted walled area of a Roman town? Gregory^{mentions}/public squares (HF 2.1; 3.15; 5.8; 6.17; 6.32; 9.9; 10.1; 10.2), draped with cloths (HF 2.31) or the scene of public appearances by a bishop (HF 2.3). What was a 'square' (*platea*) if the town was largely vacant with irregularly arranged huts? There were houses thick around the *domus ecclesiasticae* at Poitiers (VM 4.32). At Paris they were densely packed around a gate (HF 8.33) and houses covered an area between the *île de la cité* and St. Laurentius (HF 6.25). There are clear references to multiple-storied houses (e.g. HF 9.9). From this we might infer that there was difficulty in spreading outwards, that building upwards indicates cramped space in towns. Not infrequently do we hear of buildings actually built on the town walls, like the bishop of Angers' *solarium* (HF 10.14). In the time of Bishop Bertram, according to his *Testament* (c. 25), a *domus* was built in Le Mans *supra muros*.

The most telling, if circumstantial evidence for heavily built-up Merovingian towns comes (not unexpectedly) from churches. The number of chapels that could be added to the list of buildings on town walls or in town wall-towers is

considerable and is suggestive not of willfully aberrant behaviour, but that the amount of available space *infra muros* was extremely limited. Nowhere is this more explicit than in the case of Clermont. The settled area was so dense that after the building of the new episcopal church there was no room left for the episcopal palace, so the bishop had to live in an annex of the church building (M. Weidemann 1982, v.2, 78). In the fifth century, according to Gregory, the bishopric of Clermont owned very little property inside the walls (HF 2.21). Is Gregory trying to tell us that the bishop's house and church were all that stood and the innumerable little isolated churches had yet to be built, or was he explaining that the area was built up and fully owned so that, until the bishop could find willing sellers or willing donors, the church could not gain new possessions *infra muros*? Is this one reason why so many major new churches, such as Bishop Bertram's pride and joy, St. Peters, were built *outside* the town walls? Is this one reason why Köln was so ill provided with churches inside the town in the Merovingian period and not, as Steuer sees it, barren? Could it be that the scattered stray finds within the walls of Trier, which bear little relationship to the location of later documented churches, reflect burials in or by small private chapels or oratories that were not destined to survive? If we take Gregory of Tours' testimony to mean that Clermont was densely packed with secular property, that this prevented the Church from building new churches or converting old secular buildings (like the *horrea* of Köln or Trier), that the distribution of churches and stray finds from graves at Trier and Köln suggest the same problem, then we might wonder if '*sainte mérovingienne*' is such an apt description of the Merovingian town. We might ask – but has anyone? – if such numerous churches were present precisely because they were needed to hold all the faithful!

We can say that the *urbes* and *civitates* of the early medieval vocabulary were the most populous settlements in the landscape. *Urbes* were the most important and seemingly largest centres in Bede's names for places (Campbell 1979b). They were on the continent as well:

You should, however, first consider and carefully examine whether . . . the places and the number of inhabitants warrant the establishment of bishoprics. You will recall, beloved, that the sacred canons decree that bishops should not be attached to villages and small cities lest the dignity of the episcopate be lessened.

So the pope, Zacharias, wrote to Boniface in 743 (quoted in Talbot 1954, 102) after Boniface founded bishoprics in Würzburg, Erfurt, and Büraburg. Bishops were not to have their seats in unpopulated settlements. Carolingian capitularies and ecclesiastical synods repeatedly forbade bishoprics to be set in *vici*, although why

the Church felt a compulsion to make such demands is quite unclear. There seems to be little evidence of attempts to create bishoprics in villages and the translation of seats in early post-Roman Gaul was a move to more *populous* neighbouring cities, rather than one into the rural countryside. *Civitas* was, in the mind of Gregory of Tours, synonymous with a bishopric and with the physical episcopal city. He wondered, as we have seen above, why Dijon was not a *civitas* although it enjoyed the amenities of other late Roman towns, particularly, impressive walls. It was a big enough and important enough settlement to warrant its own bishop.

Now, no matter what one might hold about the urban character of early medieval *urbes*, there is no escaping the simple truth that contemporaries saw them as the largest and most populous settlements they knew, bigger certainly than any coastal trading centre. And yet, when Biddle compares Hamwih, a *villa* or *pagus* or *tun*, with Winchester, an *urbs* or *civitas*, he suggests that the former was the larger and more densely populated. As an exception to the rule one might not balk at the claim, but Biddle also suggested that the *emporium* Dorestad was more urban than London. When Hodges suggests that Tours was of the same order of magnitude as Clonmacnoise, and that both were less urban and smaller than Quentowic or Ipswich, then something has gone wrong. Wrong because those who lived and breathed in the fifth, sixth, seventh, and eighth centuries knew these places, had names for these settlements, and the names *urbes* and *civitates* were used for the largest. Archaeologists have tacitly claimed these eye-witnesses to have been mistaken.

Parasitic Towns

Without money or trade it is assumed that there simply could be no towns. Where there is detectable evidence of trade, in *emporia*, it is assumed there were towns or the prerequisites for urban growth. There is, however, another way of understanding urbanism, and one which best suits the towns of Antiquity and the early Middle Ages.

Moses Finley (1972) notes that the economic relationship of a city to its countryside can range over a whole spectrum, from complete parasitism at one end to full symbiosis at the other. Medievalists think only in terms of the latter. 'The parasitical city', wrote Finley (1972, 125) 'paid merely by returning all or part of the rents and taxes it took from the country in the first place'. He summed up ancient cities' ability to pay for their consumption as resting on four variables (Finley 1972, 139):

The amount of local agricultural production, that is, of the produce of the city's own rural area; the presence or absence of special resources, silver, above all, but also other metals or particularly desirable wines or oil-

bearing plants; the invisible exports of trade and tourism; and fourth, the income from land ownership and empire, rents, taxes, tribute, gifts from clients and subjects. The contribution of manufactures was negligible; it is only a false model that drives historians in search of them where they are unattested, and did not exist.

Fulford (1982), in an attempt to create an archaeological framework for testing Finley's claim in the case of Roman Britain, begins by noting that Collingwood and Myres (1937, 198-9) had said precisely the same thing about Roman towns. They 'were parasitic on the countryside, returning little for the food they consumed and the expenditure which they demanded for the upkeep of their public services.' Like Finley, Collingwood believed that their industries contributed only to a small extent in the production of goods needed in the country. Fulford concludes that for large towns the parasitic relationship is correct, although manufacturing ought to be emphasised more than Finley or Collingwood allowed.

Fulford is surely correct that manufacture was not 'negligible' and archaeology can prove its existence. Finley was overstating his point, but for good reason. Manufacture was not really important for the overall economy of towns. From his introduction of the parasitical city to his summation, however, Finley talks too little about the income from land in terms of the great landlords like Cicero and Pliny. Much of Finley's *The Ancient Economy* is spent developing the theme that in Greco-Roman society agriculture ennobled, but in terms of 'gentleman farming'. Cicero's preferred occupation of agriculture was really a non-profession, his estates allowed him to be lawyer, orator, and politician, not a farmer. It was this private exploitation of landed resources as much as municipal control of tributes and tolls that paid for urban consumption. Fulford (1982, 417) effectively makes the point that exploitation of their *countrymen* by landlords provided the essential fuel that fed towns:

While the presence and use of public buildings, temples, baths and defences may be a reflection of the wealth of the *civitas* as a whole, as derived from taxation, the extraordinary wealth of town-houses is surely mainly connected with the income derived from rents and rural estates. Silchester, for example, which still has the best example of a town-plan boasts more than 30 houses which could pass as villas in the countryside.

This was surely also true of early medieval towns.² The *Capitulare de Villis* (c.28)

2. Richard Hodges (1988, 1-2 for following) explicitly sets out to see if the early medieval town may be defined in the same way as Moses Finley defined the ancient city. He rightly portrays Finley as holding 'that the ancient city was primarily a consumer-city in which the economy and power relations within the place rested on wealth generated by rents and taxes flowing to and circulating among town-dwellers.' Hodges then adds as a gloss, which he admits to being his own opinion, that this ancient town was a 'corollary of the political system: a mechanism for integration of a vast polity that was barely sufficient to

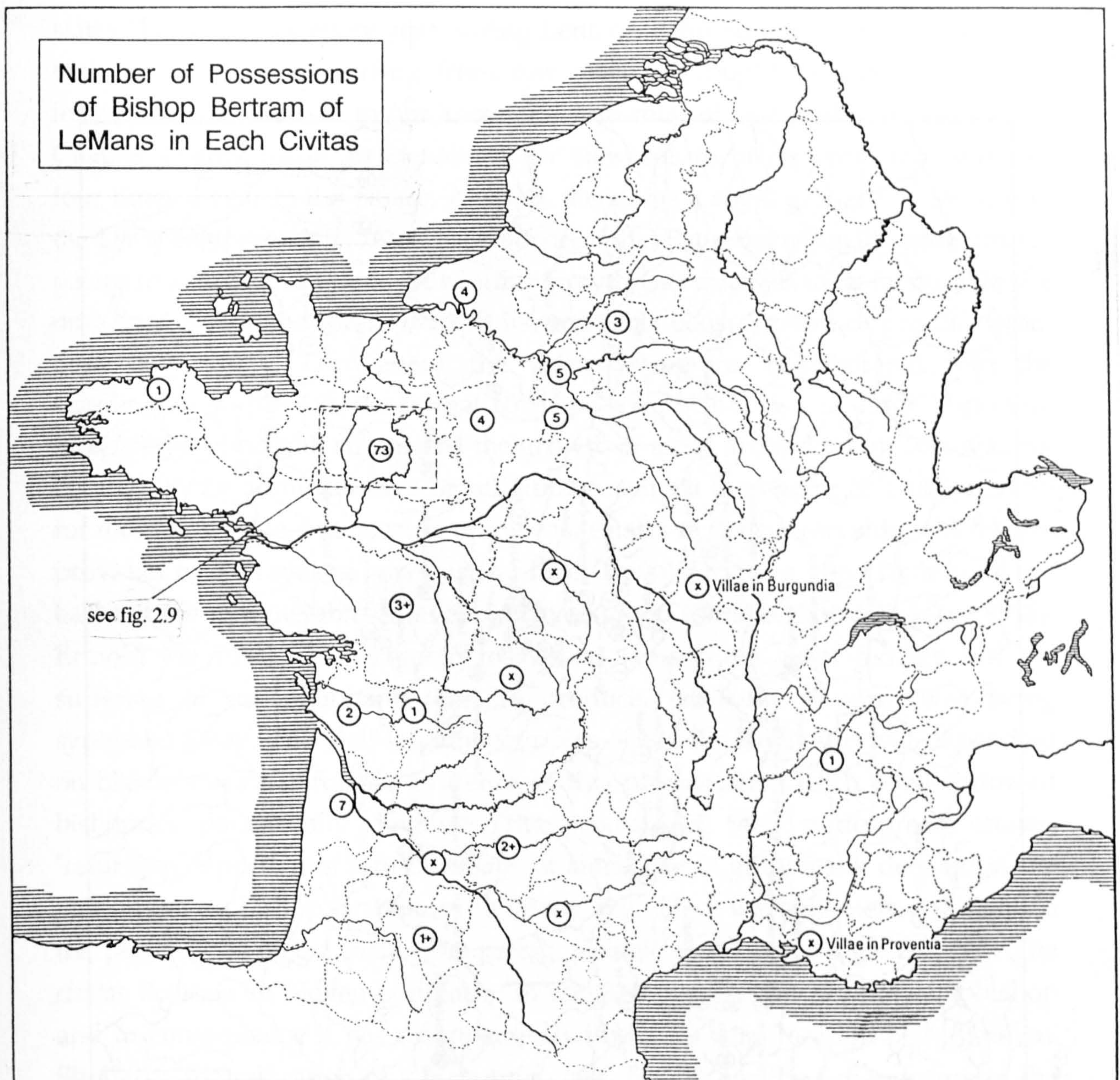


Fig. 2.8 The number of villas or possessions of Bishop Bertram of LeMans in each civitas (after M. Weidemann 1986).

meet the need of handling and processing the information flows within the Roman Empire.' But somehow this gets almost immediately ascribed to Finley: 'In Finley's opinion the ancient city is the product of its immense imperial context. . . . It follows, therefore, that the decline and fall of the context is bound to involve the decline and demise of the institution.' If the ancient town were truly no more than a bureaucratic mechanism for the emperor's authority and power, then we must surely agree with Hodges that the collapse of the imperial hegemony over Europe, North Africa, and parts of Asia Minor would see the collapse of the town. However this most certainly is *not* what Finley argued. The proof, inescapable in its simplicity, is that Finley lumped the ancient Greek *polis* with the Ancient Roman *urbs*. The little independent city-states of Greece were as far removed from the function of 'processing information flows' to integrate a vast political empire as could be. Finley's ancient city was *not* 'the product of its immense imperial court', as we have just seen.

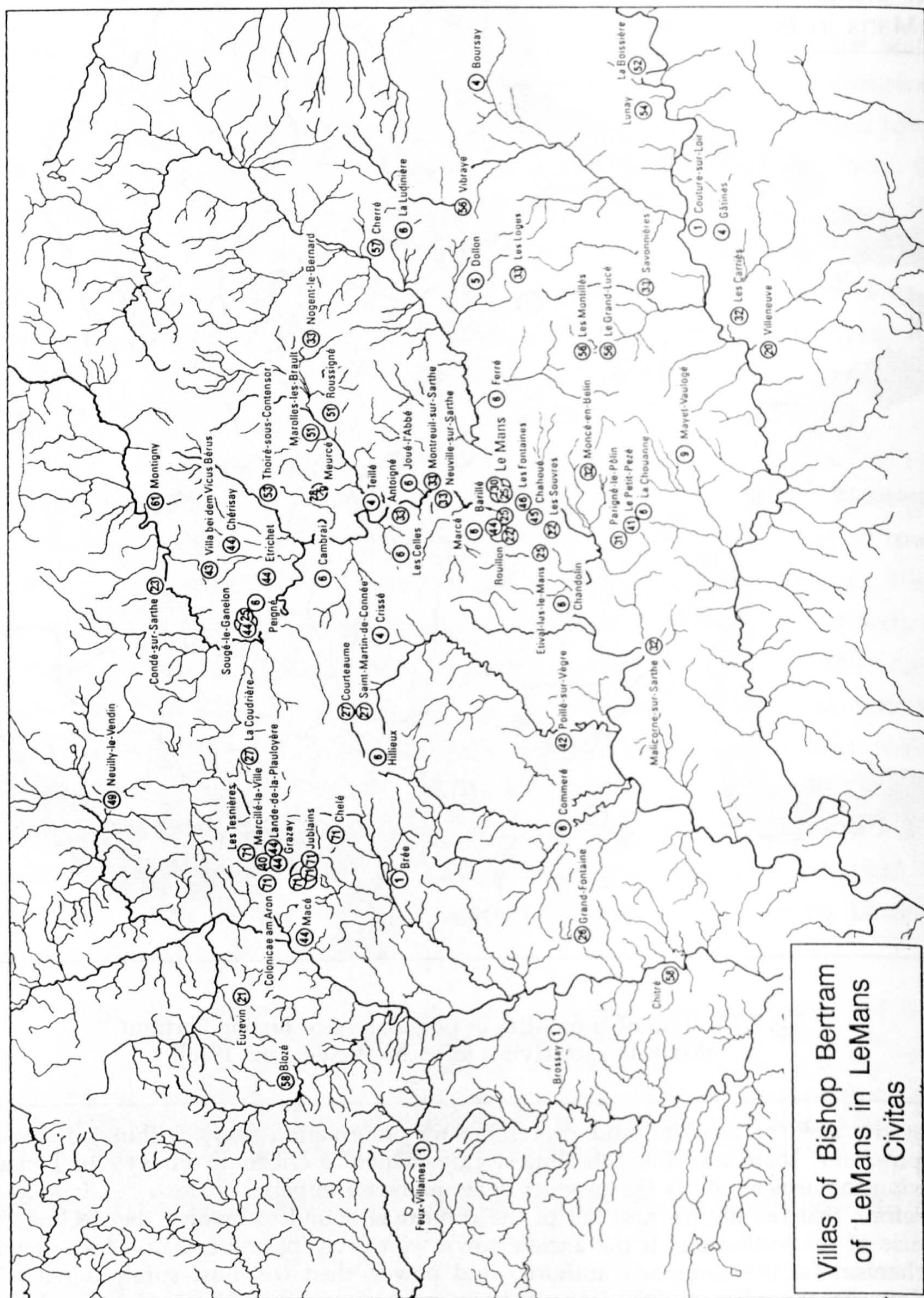


Fig. 2.9 The villas of Bishop Bertram of LeMans in the civitas of LeMans
(after M. Weidemann 1986).

states: 'We wish that every year during Lent, on Palm Sunday, stewards shall see to it that the money arising from our sales is brought in according to our instructions, so that we might know the sum total of our profits for that year.' Chapter twenty made arrangements for stewards to bring provisions three or four times a year to the palace. At Metz there was a royal granary, according to the *Life of Saint Arnulf* (c. 20). It is no surprise that Paderborn quickly grew from a palace to a town and Frankfurt's initial growth was due less to its favourable site on a ford across the Main, than to its insatiable consumption of produce from dependent villas. Throughout the German regions incorporated into the Carolingian empire one finds that royal palaces, bishoprics, or large important monasteries almost all stimulated the growth of a city around them. This was *not* because of the administrative or religious functions they were able to 'provide' for the countryside, but because of the vast estates in the countryside which were provided to the royal or ecclesiastical lord. Thus Würzburg grew rapidly on the backs of the innumerable villas and manses gifted to the bishop (Dinklage 1951). Ermold the Black, in his poem to Alsace, laments its great fertility but the suffering of its inhabitants, for its produce, particularly wine, was being syphoned away. The well-known examples of Roman town survival dependent on bishop's seats is further evidence of this phenomenon. Rich, well-endowed bishoprics parasitically devoured the produce of their widespread estates, 'returning' a portion of it in the shape of alms. This is graphically detailed in the work done on Bishop Bertram of Le Mans' will. Here were the new additions to the possessions of the bishopric during a single episcopacy (figs. 2.8, 2.9). The *civitas Boiorum* in Novempopulana in south-western Gaul never had a bishop and in consequence it has disappeared without its very location being known. Similarly, the relocation of a bishopric spelt disaster for the urban future of the former sites, for the wealth extracted from the rural hinterland by the bishopric's endowments was consumed elsewhere. The bishop of Aps moved to Viviers in the sixth century and the fortunes of the former consequently sank. Bishops similarly moved from St-Paulien to Le Puy and from Javols to Mende with similar results.

The contribution of 'fuel' from the estates of nobles is suggested by Gregory's mention of houses of *senatores* in Bourges (HF 1.31) and Tours (HF 10.31). No less than twenty-two different named *cives* of Tours appear in Gregory's anecdotes throughout all of his works and such people were clearly important laymen. A rich man living in Comminges with his cellars full of grain (HF 7.37), and two of the *cives* of Tours were expressly called *domini* and owned slaves (VM 2.13, 3.29). Town houses kept by nobles, as one might expect, are found in Sidonius

Apollinaris's correspondence (*Epis.* 2.12.2; 3.3.5; 7.15.1). The Pippinids seem to have kept a house in Metz (Pardessus 493). A noble woman, Ermentrude, who resided in Paris made diverse and substantial gifts to the various churches in the city (Doehaerd 1971, 120). Bishop Bertram left a house inside the walls of Bordeaux and Le Mans to his nephew Sigechelmus (*Testament* c. 25; 30), thus documenting a desire for such property among the laity. Indeed, the properties of which Bertram disposed in his will, including all of the *infra muros* properties left either to the church or to his family, ultimately derived from a secular source. Here we see continuity from the fifth century, for although there was a marked move by the powerful in late Antiquity from towns to their estates, nobles continued to have town-houses (Sidonius Apollinaris *Epist.* 2.12.2; 3.3.5; 7.15.1). Brian Ward-Perkins (1988, 23) gives this as one of the reasons for the 'resilience' of early medieval Italian towns:

towns almost certainly remained popular places for the secular aristocracy to live, as in Roman times. Again, so far, this is clearer from historical rather than archaeological evidence. In particular the private charters of the Tuscan town of Lucca, which survive from the beginning of the eighth century onwards, reveal an urban-based landed aristocracy buying, leasing and selling lands in the surrounding countryside, and founding churches and monasteries within the town. . . . In economic terms . . . towns in Italy continued to serve an essentially negative role, as major centres of consumption: living off the surplus of the surrounding countryside, rendered by peasants in the form of dues and taxes to urban-based landlords and to the rulers of church and state.

As primarily consumers of rural production, early medieval towns had much in common with Roman towns. Medievalists often fail to recognise this, believing that Roman towns were largely thriving centres of manufacture and trade. The growing commonplace recognition that towns began their decline at the end of the third century, following the great Germanic barbarian incursions is symptomatic. The fall of the empire is thus seen as inevitable, for the slow disappearance of great public buildings and amenities is taken as evidence of economic failure. Nothing could be further from the truth. 'By about 300 a dramatic change had taken place' (Ward-Perkins 1984, 14) in towns, but the change was not economic, it was in the manner by which individuals gained status and imperial posts. Secular munificence belonged to a system of competition for civic magistracies. Increasing imperial control meant that officers were increasingly appointed rather than elected. Therefore the wealth extracted from the countryside by rich landowners was no longer expended conspicuously on home towns in bids for office. The money found new outlets in the luxury of private villas.

The withdrawal of Roman landlords on to their estates is well known (e.g.

Wightman 1978b). The move also had effects on the manufacture and marketing located in towns. Finley (1972, 140-1) summarises succinctly:

When wealthy absentee landowners withdrew to their estates, they tended to convert their new bases not only into fortified centres [questionable] but also into self-sufficient communities, supplying as much of their own needs as possible, in food and clothing, in woodwork and even metalwork. These men of course continued as commodity producers . . . but they appear to have reduced the market as a whole by their change in residence, which amounted to a change in way of life.

This change from secular munificence and civic competition to private luxury and imperial appointment in the late empire is of enormous importance for the understanding of towns, although as yet it has not received the interest it deserves. Ward-Perkins (1984, 194) notes that 'whereas in classical times the work of building and repair seems to have come from normal civic income or special gifts, in late antiquity it came from forced contributions of money, or labour specially levied for the purpose.' The change was a necessary result of the abandonment of civic donations fuelled by competition for local offices. It was the beginning of compulsory road-, bridge-, palace-, and wall-work that would survive in Italy and be revived if not continuous, in Gaul and Britain in the early Middle Ages.

The *Liber Pontificalis* (quoted *ibid*, 195) says of Pope Hadrian I's wall-building efforts at Rome that

Through careful effort he assembled the men of all the cities of Tuscia and Campania, as well as those of Rome and its district, and of all the ecclesiastical estates (*tota ecclesiastica patrimonium*). He divided the wall into lengths, provided papal funds and food, and thereby renewed and embellished the whole city by restoring its wall.

Nicholas Brooks (1971) traces the first appearances of the so-called triple necessities of building bridges, burhs, and roads in Wessex in the mid-ninth century and in Mercia in the mid-eighth century. Southampton, however, reveals evidence of planning in its late seventh or early eighth century foundation; the credit for planning is often given to King Ine. Forced labour was probably involved in laying out streets at Southampton as it was in repairing the walls of Rome. Reece (1980, 89) is correct: a church and a palace amid fields of grass do not make a town. But where does one find impotent kings and bishops sitting in the grass, side-by-side but otherwise alone in a deserted town? Towns in post-Roman Gaul were not an ill assortment of lean-to huts and their continuity was neither tenuous nor based on superstition or the 'need' for a few wretches to huddle together for company or safety. Kings and bishops had resources, vast resources, of labour and kind, and it was the exploitation of that labour and

income derived from the royal fisc and even more from the estates and tithes collected by bishoprics and great monasteries that built early medieval towns, not a mercantile economy. Those old Roman towns that did best, like Soissons and Reims, had both bishopric and a large important suburban monastery. If it was not a capitalist, monetised economy of production and trade that created these towns, but the exploitation of rural farmers, then urbanisation was no flame in danger of being extinguished. Important royal villas such as Thionville or Aachen, even when in a quiet rural setting, generated urban growth. Compiègne nearly suffered the indignity of having its name changed to Carlopolis by Charles the Bald, but his was no attempt at founding a city *de novo* in Roman imperial fashion. Compiègne's industrialisation today owes much to the fact that Charles the Bald chose it as his favourite residence, and like all important royal Carolingian villas, it was furnished with the products of innumerable rural estates, tribute, plunder, and cash from tolls and taxes. Nor was this the only source of rural exploitation that fuelled Compiègne's growth. In 877 Charles the Bald founded a monastery dedicated to Mary, explicitly with Aachen as a model. Streich (1984, v.1, 47) lays great importance on the Aachen model although admitting that the practice had already been common in Italy. We will see, however, that the practice was not uncommon among the Merovingians. In fact, the successful growth of such foundations, because so well endowed, often led to a later abandonment of the royal palace and its appurtenances to the monastery. Contributing to the massive consumption were the monasteries' estates, revenues from dependent churches, tithes, and even a tenth of the produce of certain royal estates. Thus we know many of the royal estates in the Ardennes from the register of tithes that they paid to St. Mary in Aachen (Müller-Kehlen 1973).

Emporia could not compete with this massive consumption of exploitatively collected wealth. To scoff at Kildare and Tours as no more than a community of monks and their attendants, is to overlook that their economic power derived from their social and economic capacity to exploit their dependants not from capitalist enterprise. Georges Duby (1974, 106) writes: 'What was Duurstede? Archaeological investigation has revealed it as a narrow street, one kilometre in length: a road lined with warehouses wherein a few traders, for whom a parish church had been erected, lived as permanent residents.' If Dorestad had been 'the greatest town west of Constantinople' it did not provide for the spiritual needs of the heaving masses in the way a normal *ville sainte mérovingienne* did. Verwers (1988, 55) suggests that from the cemetery of 2,350 graves the number of inhabitants in Dorestad at any moment was probably under two thousand. This is the sort of population one might expect at a large urban extramural monastery in

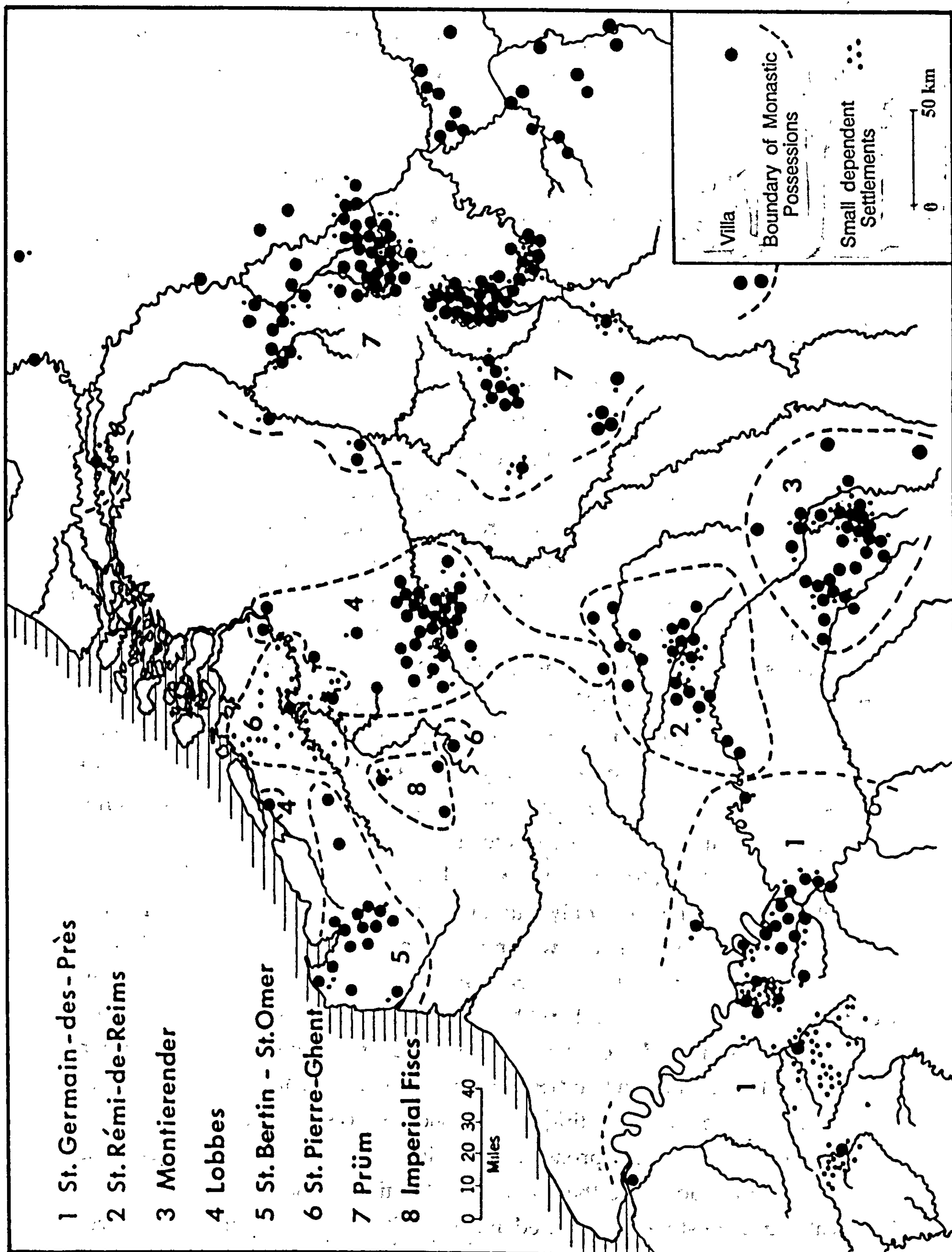


Fig. 2.10 Villas and their dependent peasant settlements possessed by seven large Carolingian monastic houses in northern Gaul (after Pounds 1967).

Gaul, not to mention the rest of the city's inhabitants.

Not counting the food and cash renders, by 825/6 Fulda, with at least six hundred monks, could count on half a million day-works per year from its dependent peasantry! Hariulf's *Chronicle of the Monastery of St. Riquier* reveals just how populous and urban a powerful monastery could become in a short time. Angilbert, a friend of Charlemagne, by whose daughter he had two children, founded Centula which, according to Hariulf, had 300 monks, 100 scholars, and 2500 *homines saeculares*. Angilbert's own religious offices were written down and thus a contemporary description is given of a procession which passed by the many *mansiones fabrorum* by the gate and *iuxta murum* – the monastery's profane quarter was enclosed. These secular inhabitants included *milites*, merchants, smiths, shield makers, saddlers, furriers, textile producers, butchers, bakers, cooks, vintners, and brewers, each trade described as being in its own *vicus*, or quarter. Each owed renders peculiar to their trade and each *mansio* owed twelve pence, chickens, eggs, and labour services. Lesne (1943) long ago revealed how 'population' and 'exploitation' were related and how both were inextricably bound with the organisation of monastic centres. Vast areas of the rural countryside in northern Gaul were worked for the profit of seven major Carolingian monasteries (fig. 2.10). It was not 'religious – one might even say superstitious – attractions' that prevented such centres from becoming depopulated. Kuchenbuch (1978) investigates the social structure of the 'familia' of Prüm abbey in the ninth century with the telling title of 'peasant community and monastic lordship'. The particularly rich documentary sources allow him to discuss the conflict in some detail; the peasants scarcely appear as simple creatures held bound to the spot by superstitious credulity. Gregory of Tours may have many anecdotes of St. Martin's miraculous powers and it is hard to imagine his contemporaries, be they only humble peasants, as being less superstitious than he. Nevertheless, if Tours was more than ramshackle huts and churches strewn sparsely, it was because St. Martin's monastery was probably the wealthiest in Merovingian Gaul and had the most dependants, but not because St. Martin's tomb overawed credulous rustics. In Charlemagne's time, Alcuin was abbot of St. Martin's and three other monasteries which made him responsible for great wealth. Too great, according to the bishop of Toledo, who reproached him with having 20,000 *servi*; a reproach Alcuin did not deny, but rather insisted that he himself had not added to the number. St. Martin's monastery, not to mention Tours cathedral, must have possessed at least 5,000 *servi*. It was this vast reservoir of exploited labour that made Tours so much more important a centre than any *emporium*.



Fig. 2.11 Bonn, showing the concentration of early medieval churches and burials around ecclesiastical property outside the Roman castrum (after Böhner 1978). The slightly larger enclosed medieval town was one kilometre south of the castrum.

The growth in the number of monasteries and their increasingly important economic role is chartered by Higounet (1960), who counted 196 monasteries in Neustria, 195 in Austrasia, 153 in Aquitaine, and 92 in Burgundy and Provence for the year 800. Again through the underlying assumption that urban existence was dependent on a capitalist economy, it is often overlooked that Carolingian settlement geography did not become more rural because some of the major 'urban' centres were at their core monasteries, such as Calais or St. Quentin, while many an old Roman town, especially without a bishop, sank into obscurity. In fact many of the changes in the urban landscape were little more than reflections of shifts in the political-economic landscape. Such shifts are recognisable even in the topography of individual towns, as in the case of Bonn (fig. 2.11). The time has come for the recognition that the shift in the centre of such sites was the result of social and economic dependency and not the touching desire to live by the physical remains of a beloved patron saint.

Whatever the mechanisms of exchange, it was in towns that much was consumed and economic activity was most intense. By Hodges' own insistence, *emporia* grew as the result of elite dominated and oriented consumption. It was not at the *emporia* that this consumption occurred, but at palaces, monasteries, and towns. It was not at *emporia* that the goods were produced, the wine came from estates along the Rhine, Rhône, and Garonne. And was it estates of the Cotswolds that produced the wool to which Biddle (1976) credits the origin of this *emporia* trade? Hodges' *emporia* were important elements in the system of production and consumption, as places of exchange of exotica, but they were peripheral to 'the origins of towns'. The situation was to change little until the ninth and tenth centuries. This was not a *new* beginning for towns, but the first real beginnings of towns with a symbiotic relationship with the countryside. Manufacture and mercantile wealth was slowly becoming based on the exploitation of urban workers not rural peasants.

There is no disguising the fact that there was an urban decline in the post-Roman centuries. In Italy apparently 116 of 372 Roman towns disappeared between 300 and 800 (Ward-Perkins 1988, 16). The darkness of Dark-Age archaeology is largely the result of widespread use of perishable building material, in contrast to the general use of stone, brick, and mortar in the Roman empire. The decrease in the number of former Roman towns still in existence in the early Middle Ages is partially offset by a number of new monastery or royal palace towns. However, the general impoverishment of urban life remains clear. How do we explain the

decline, what were some of the causes?

The anti-Pirenne advocates of 'trade makes the city' would point to a collapse of a 'world system', the failure of organised minting, disturbances in the peaceful conditions and political stability that enabled long-distance mercantile trading, and thus the loss of markets. Doubtless this must have had some effect. My own thesis of continued parasitic exploitation of the countryside through tithes, taxes, tribute, rent, and labour does not deny a role for trade, sale, and markets. It does, however, demand either that one denies the decline of towns was really drastic or that there were further reasons than simply the failure of markets and small coinage to continue after the Roman empire fell. As the empirical evidence for decline is too strong to argue away, my adapted Marxist-Finleyist parasite thesis will necessarily have to accept that another cause lay behind the wane of urban centres.

The most simple and appealing reason is that which is most in keeping with the tenets of the parasitic thesis. In short, there was less exploitation of the rural countryside than before. What form might this have taken? The amount of taxation by the state raised from landlords and peasants fell. Less rent or renders were extracted from peasants because the conditions of their dependency lightened; slaves became serfs, *coloni* became rent-paying tenants, or rent-payers became independent farmers. The exploiters became more dispersed and less frequently gathered in towns; landlords stayed on their estates. Or the number of great landlords decreased and the extent of their possessions diminished; the scattered empire-wide estates of the fabulously wealthy aristocrats of the late fourth and early fifth century were broken up between many new lords, Germans, provided with 'hospitality'. In short fewer, less wealthy lords and masters exploited peasants and slaves less extensively than in Antiquity. This is probably the most important cause in the deterioration of towns, not a drastic reduction in the total population, nor the collapse of trade and markets.

The much more marked survival of towns in Gaul than in Britain, therefore, implies the more general survival there of estate-owning, peasant-exploiting lords, whose residences were to be found in both the town and country.

Vici: the Villages

Smaller than the *urbes* were the *vici*, villages. Those of Merovingian Gaul have been little studied, which is not surprising given that their predecessors are little better known. 'It has to be admitted', wrote Wightman (1981, 238), 'that the function of the *vici* within the socio-economic structure of [Roman] Gaul is as yet only partially understood.'

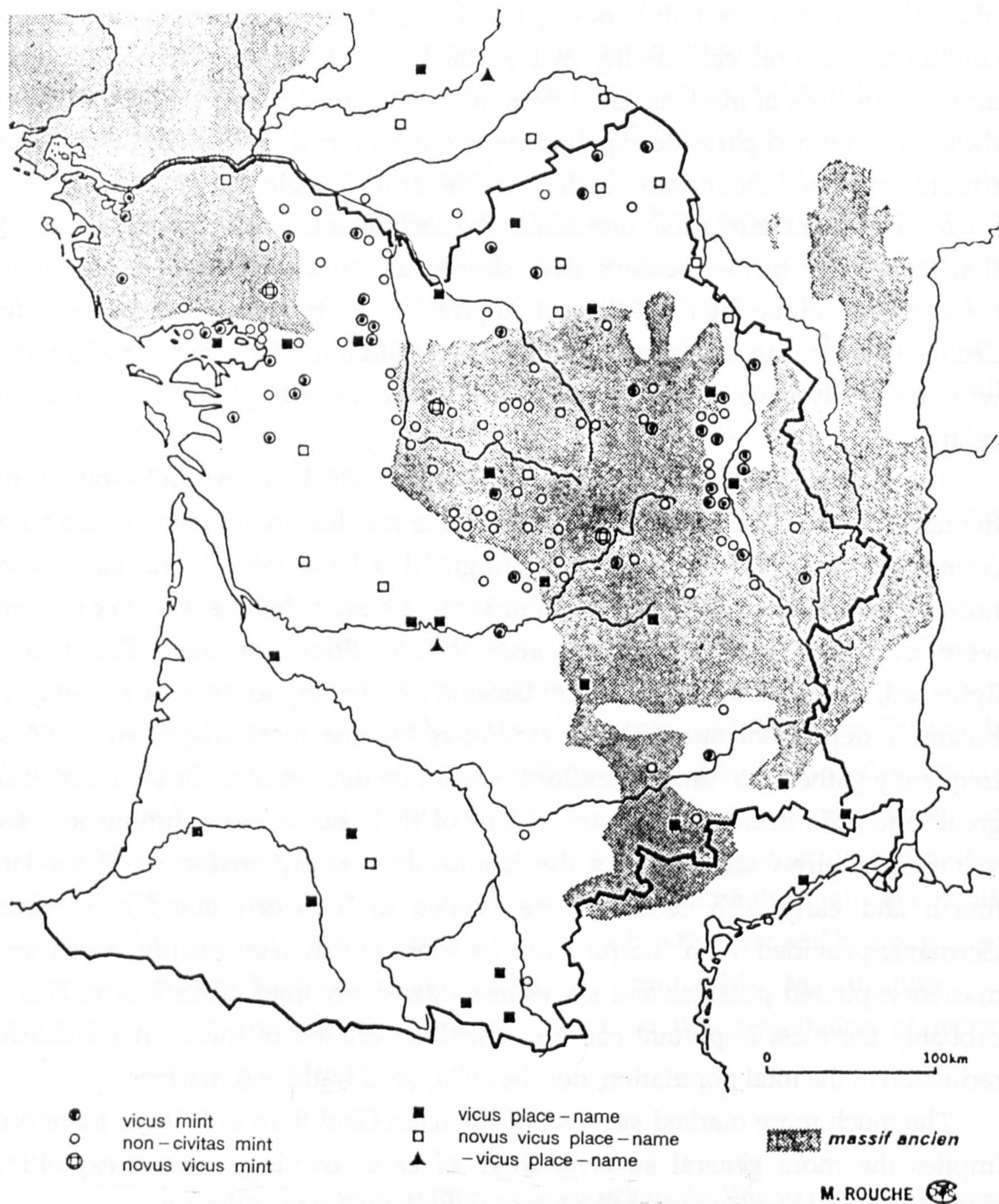


Fig. 2.12 The vici of Aquitaine recorded in place-names and on coins (after Rouche 1979).

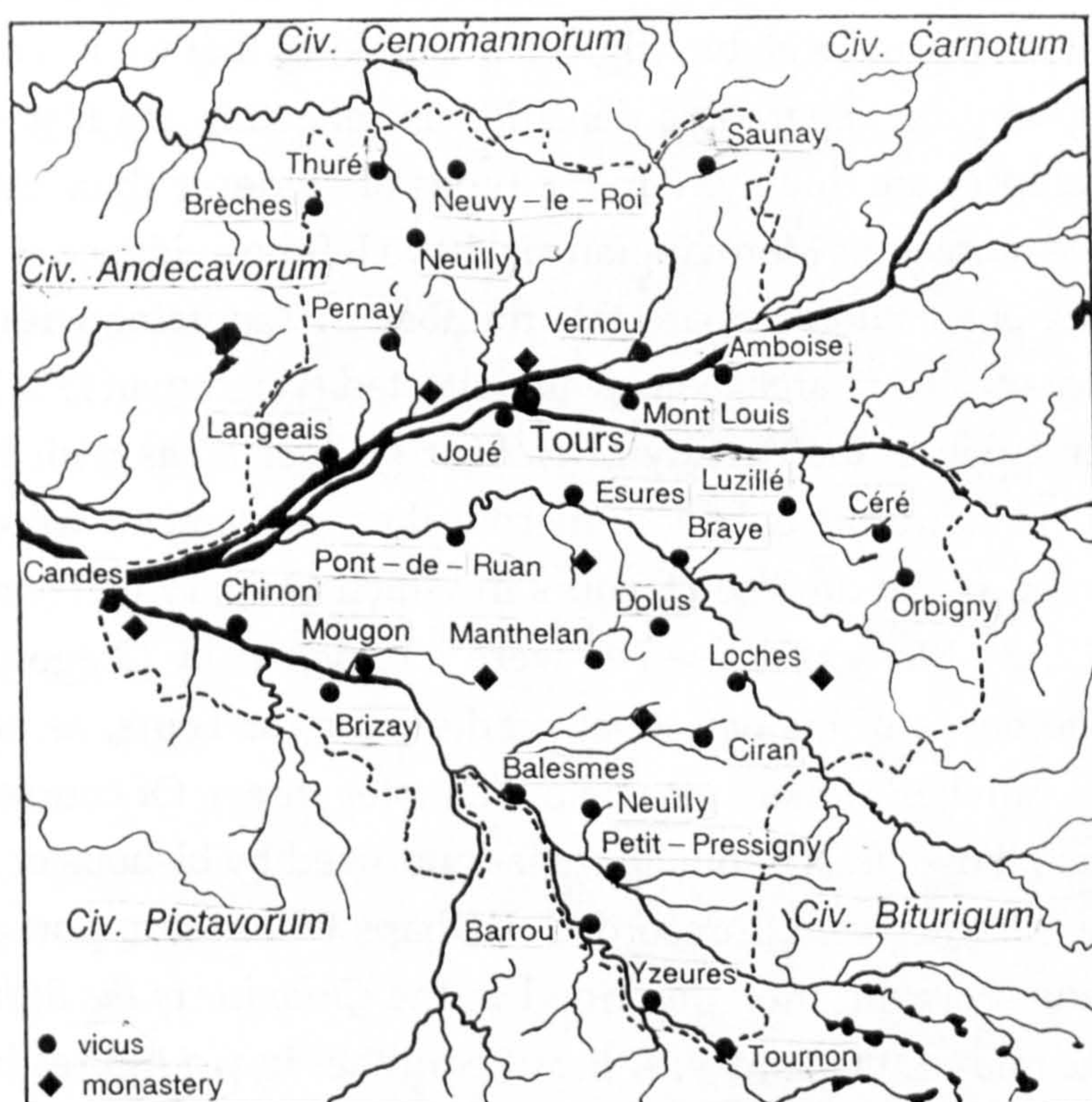


Fig. 2.13 The vici of Tours civitas mentioned by Gregory of Tours in the sixth century (after Longnon).

Long ago Fustel de Coulanges (1889, 198-220) suggested that Merovingian *vici* were no more than villas, based on the mere seventeen he found among all the Merovingian charters. Robert Latouche (1961, 65) was rightly scandalised, for *vici* appear in much greater numbers in sources other than the charters, and French scholars continue to berate Fustel de Coulanges for this oversight (e.g. Rouche 1979, 221). Perhaps a century on we can forgive him and move on.

That *vici* were numerous in Merovingian Gaul is revealed by the number that appear in Gregory of Tours' writing alone: about 70 (Longnon 1878, 72; Weidemann 1982). They also figure on numerous coin legends, most often with abbreviated name and VIC or VICO (Prou 1892). The named but undesignated settlements on many other coins were probably *vici* rather than *villae* (we assume we know the names of all the *civitates*); we know of no mints at sites termed *villa* by any sources although there are a few *palatia*; settlements regularly designated *vici* often are documented by their name only; and a few of the named mints can reasonably be ascribed to settlements termed *vicus* in a textual source. Therefore, we can add considerably more names to our list. Using all available sources of evidence – named *vici* in documents, coins bearing a *vicus* epithet or simply a

name, and place-names containing *-vicus* – Michel Ruche (1979) has mapped all the probable early medieval *vici* of Aquitaine, numbering about 150 with an additional 20 being unlocatable (fig. 2.12). He notes that we must be wary about interpreting varying geographic densities as reflecting the historical condition, for the differences are due more to the types of evidence than anything else. No doubt a large number of Merovingian *vici* have left no evidence of their existence. Nevertheless, one could compare this number in Aquitaine with the 87 Roman *vici* of Belgica which are archaeologically attested (Wightman 1981).

A better guide to the density of *vici* are smaller areas with fuller evidence. One of the best indicators of how numerous these settlements were has long been Longnon's map of the diocese of Tours in which Gregory of Tours named some 32 *vici* (fig. 2.13). Many of these *vici* were mentioned by Gregory in his potted history of the deeds of his episcopal predecessors at Tours, as he recorded the founding of churches in them (see below chapter three). Of course this is still far from a full list; those *vici* without churches endowed by bishops or not the sites of newsworthy miracles went unrecorded. Perhaps the most important guide is the parochial church organisation described in the *Chronicle of the Bishops of Le Mans* written in the mid-ninth century. A list of churches in *vici* figures in the chronicle as owing an annual quota of silver, wax, and oil. Calculated in triens, the list must date to the Merovingian period. The list contains ninety *vici* in the diocese of Le Mans alone.

The Merovingian *vici*, one presumes, were descendants of Roman *vici*. Such a presumption must be accompanied by the acknowledgment that we are on doubly unsure ground, for we know little enough about these settlements in either period. The built-in assumption of continuity heightens the danger of circularity. Thus for instance Leday produces a list of 25 Roman *vici* for the Berry, but this is to a great extent the product of early medieval sources. However, aerial photography offers more security to our assumptions: 'As a general rule, the *vici* of the Bituriges developed into modern centres of population, thus a number of sites lie under existing towns' (Leday 1980, 305). Often the documentary sources are sufficiently full that we can be certain of continuity. Gregory of Tours' interest in his Auvergne homeland provides us with anecdotes that dovetail into those provided by Sidonius Apollinaris a century earlier. There is no doubt that some centres like Brioude were Roman in origin. Excavation at St-Martin de Mondeville reveal striking continuity of *Grubenhäuser* from at least the third century throughout the early Middle Ages (Lorren 1981; 1982; 1985). A small village-like settlement, it might be deemed a *vicus*.

Clearly not all Roman *vici* remained occupied in the Merovingian period.

Wightman (1981) calculates that 31 of 87 archaeologically attested *vici* in Belgica did not survive the late third century, just over a third. Those that survived the third century crisis were perhaps more firmly established geographically, economically, and socially. The fifth and sixth centuries perhaps proved less critical. Clearly not all Merovingian *vici* were established in the Roman period. We know from written sources that some *vici* were supposedly new foundations. Although there is always the fear that the praise for the founder was exaggerated, we do know that the creation of new *vici* was no invention of writers' imagination, given that in Aquitaine alone Rouche can map eighteen settlements named *novus vicus*.

It may be that site continuity is partially to blame for how little we may claim to know about Roman *vici*; few were abandoned to leave optimal excavation conditions. It may be that modern scholars have simply failed to find a niche for the *vici* in their interpretative schemes, and so they languish. A conference in 1975 on 'small towns' in Roman Britain (Rodwell and Rowley 1975) revealed how little was known of these sites, even down to the almost total lack of knowledge of what they were termed by contemporaries or how they were administered (Johnson 1975). Fulford (1982) refused to pass judgement on whether, like *civitates*, these 'small towns' were parasitic because so little is known of their economy. Johnson (1975) was able to show that the *vici* of Gaul were slightly better evidenced. Magistrate offices are at least documented, although whether there was an autonomous council running the village's affairs is not known. There are further difficulties, such as the absence of *vici* from the *Itineraries*, although *mutationes* and *mansiones* are mentioned alongside *civitates* and *castra*.

The traditional function assigned to *vici* is that of the staging-post, reflected in the French term *bourg-routiers* for these sites. Frequently found on roads, especially at cross-roads, they are taken to have contained the *mansiones* in the itineraries and to have served both the *cursus publicus* and private travellers. To this extent it might not be unimportant that Dill (1926, 235-67) stressed the evidence for the continued good repair of Roman roads in Merovingian times. More important is the place of the *vici* in terms of local markets, manufacture, and agricultural production. Wightman (1985, 94-5) saw all these functions in the *vici* and concluded that it

is tempting to see in the villagers numbers of small men making good, practicing crafts in return for monetary rewards and thus freeing themselves from the constraints which an earlier form of traditional life and patronage imposed on them.

This has been taken up by medievalists. Typically we find Rouche discussing

vici under the sub-heading '*La petite propriété et les vici*'. His conclusions are that small proprietors and groups of free peasants in *vici* were common in Aquitaine, more so in the north than the south. This distinction he attributes to the greater extent and power of aristocrats in the south with their large estates; they inhibited the growth of *vici* or even took them under their patronage. For the same reason Latouche vehemently denounced de Coulanges' portrait of the Merovingian landscape dominated by cities set among a continuous rural countryside of large villas. To him, the *vici* were villages of free peasant farmers, although occasionally in danger of falling prey to powerful landowners and thus being converted into villas.

Are we to accept the *vici* as small islands where free peasants dwelt? What was their relationship to the surrounding rural settlements, the villa estates? Wightman (1985, 95-6) noted that Roman villas are often found within a kilometre of the *vici* 'which tells against the cultivation of large areas of land by the villagers, as does the comparative rarity of agricultural implements and the unsuitability of the average *vicus* dwelling for the storing of vehicles or the stabling of larger animals.' This might reinforce the idea of artisanal manufacture or very small-scale farming, for there is plenty of room for gardening, even market-gardening suggested Wightman. But is it the only possible way of interpreting the *vicus-villa* relationship?

Malcolm Todd (1988, 17-9) offers an alternative:

Large estates of the later Roman Empire in particular will have required large numbers of workers, quite possibly entire communities of them, and yet the archaeological record of Gaul and Britain does not reveal extensive buildings close to known villas in the great majority of cases. Now that the immediate environs of a number of villa-sites have been more fully examined, on the ground and from the air, we can be somewhat surer about this than before. Where, then, were the estate-workers housed? One obvious and convenient solution was to establish a communal settlement on the estate, an arrangement which seems to have been first observed in Gaul nearly a century ago. In Britain we might see at least some of the minor nucleated but still essentially rural settlements which have emerged over the past fifty years as a significant element in the Romano-British countryside in this light. . . . It is certainly difficult to fit these often sizeable and sprawling rural townships or villages into any kind of social framework without seeing them as elements in a system of estates. Not all can be *pagus*-centres, and that they housed a free peasantry has never seemed plausible. Indeed the harder we look for a free peasant in the western provinces, the more elusive he becomes.

Todd points to the well-known documentation (*Agrimensores*) of disputes between municipal authorities and private individuals in Africa, which relate that single landowners might have villages surrounding their villas, as though they were '*municipia*'.

This might explain the seventeen *vici* of the Merovingian charters recognised by de Coulanges, but their very infrequency suggests that no one landowner regularly had proprietary rights over such settlements.

It may also be that, despite their not inconsiderable number, they were relatively unimportant to the total settlement picture. That is to say, perhaps the *vicani* represented only a tiny portion of the total population.

The work of Agache (discussed in greater length in the next chapters) in the Somme, has created the impression of a countryside regularly dotted with villas in the midst of which were large towns but settlements of an in-between size, villages, are comparatively rare. This recent archaeological picture ironically mirrors de Coulanges' old textual view of the Merovingian landscape and not that of Latouche.

Castra: Villages or Castles?

Castrum and *castellum* are potentially very interesting for a thesis concerned with the dwellings of the social elite, for the word was used regularly by later medieval scribes to describe what we would call a castle today. Of course, that was not for another four or five centuries in the Merovingian period, and even then the terms would be used of sites we would call towns or episcopal precincts as well (Coulson 1973, 66). In translations of Merovingian texts, one regularly finds the terms rendered as 'castle', and specific work on the Merovingian use of the terms (Viellard-Troiekouroff 1978) and archaeological work on sites so termed (Fournier 1974; 1978) explore their relationship to later 'castles'. Alas, the sites referred to as *castra* and *castella* by Merovingians were nothing of the sort (like early medieval clerics, I use the two almost synonymously, preferring the former).

The distinction between *civitates* and *castra* in Merovingian terminology was simple. *Urbs* or *civitas* referred to a city with a bishop. With only two exceptions, Deutz and Zülpich, every *civitas* mentioned by Gregory had a bishop. Longnon suggested that these two had ephemeral bishoprics, as Gregory informs us, had Champtoceaux, Tonnerre, and Châteaudun. An equally plausible explanation is simply that Gregory made a mistake. Arguing further for this distinction we can note that the *castra* Mâcon, Chalon-sur-Saône, Uzès, and Carcassonne of the *Notitia Galliarum* and *Bordeaux Itinerary* were 'up-graded' by Merovingian writers to *civitates* and all three possessed bishops.

Dijon was not alone in having late Roman walls and being designated a *castrum* in Merovingian terminology. Koblenz, Tournus, Melun, and Beaune were analogous. Dijon and Koblenz enclosed areas of more than ten hectares, but

Tournus, Beaune, and with little doubt, Melun, enclosed only two hectares or less. These smaller towns clearly lacked bishops simply *because* they were small. Gregory showed surprise that Dijon lacked a bishop, perhaps he would have done so of Koblenz had he known the town.³ The urban nature of these sites is undeniable. It is hardly surprising, therefore, that Vieillard-Troiekouroff (1978) should have found that medieval castles owed little to these Merovingian *castra*!

What has clearly caused confusion for at least one Romanist is that *castellum* is a term used today by Romanists of fortified centres smaller than towns, most commonly between 1 and 5 ha. (it has become something of a technical term for archaeologists, just as *burgus* is used for watchtowers). They lay within the administrative region of a *civitas* and were designed primarily to house a military garrison. A civilian population was also regularly present, either inside or outside the walls. In part this was a historical accident, the result of the loss of the *agri decumates*, for the new frontier along the Rhine and Danube put a number of formerly purely civilian settlements, like Kaiser Augst on the front line. In the late empire, however, the distinction became less clear as the army produced settled *limitani* garrisons. This close connection with the army means that *castella* are found predominantly in the frontier regions, mostly in the two Germanies, Belgica Secunda, and Maxima Sequanorum. No Romanist would call Dijon a *castrum* or *castellum*, but rather a town. So common has the term become in archaeological circles that it is easy to overlook how little evidence there is for native use of the term, although Stephen Johnson (1975) does show the very suggestive evidence of several sites changing their qualification from *vicus* to *castrum* only after getting an enclosure wall.

These sites have revealed a remarkable amount of continuity, although it should be added that the attention lavished on the German *limes* has made our knowledge of this part of the empire disproportionately good in comparison to others: Neuss (Borger 1969), Alzey (Böhner 1969a), Bingen (Böhner 1969b), Bad Kreuznach (Böhner 1969c), Zülpich (Böhner 1974), Bitburg (Böhner 1977b), Bonn (Böhner 1978), Deutz (Precht 1980) (fig. 3.6). The continuity has been revealed most clearly in the funerary evidence, in itself remarkable for the continued use of late Roman cemeteries into the *Reihengräber* period which is not otherwise common. Such cemeteries are known from most *castra*. Andernach has yielded 300 burials, Basel-Aeschenvorstadt 600, Kaiser Augst 1300, and the fullest and

3. Although he quotes from Longnon, Johnson (1975, 79) oddly claims that the relationship between the two terms was not clear in Gregory's day. He refers to passage describing Dijon (quoted above p. 19), claiming that it reveals that Gregory (HF 2.23) was bewildered. He was, of course, only bewildered that Dijon should lack a bishop, and not what distinguished a *castrum* from a *civitas*.

most important excavation of a continuously used cemetery in the western empire to date, Krefeld-Gellep, has produced several thousand burials. Attempts to show that the population in such *castra* remained Roman among a sea of German settlers (e.g. Böhner 1966) have received widespread acceptance. Although I believe that there is little to recommend the underlying assumptions of these arguments, namely that material culture can be used to identify ethnicity, the studies most certainly do reveal continuous occupation along the Rhine at these settlements. In the Rhineland, evidence for continued occupation also comes from another quarter, rare for other parts of Gaul, for some of the earliest churches which survived the barbarian invasions are found in *castra* here, such as Alzey, Bingen, Kreuznach, and, perhaps best known of all, Boppard.

Elsewhere the evidence of continued use is less clear. For example, *castra* cemeteries along the Danube reveal almost no continuity (von Petrikovits 1958). Nevertheless, precisely where one would least expect continuity, along the *limes* and *agri decumates* overrun and abandoned in 260/1, Weidemann (1972) has revealed that at no less than 35 sites, finds post-dating 260 have been found. It is not surprising, therefore, to find Andernach, Bingen, Bonn, Bregenz, Breisach, Boppard, Deutz, Koblenz, Neuss, and Zülpich all termed *castra* in early medieval sources (Köbler 1972, 20). Nor is it surprising that small towns with no more or even less area enclosed behind walls such as Tournus, Beaune and Melun should be similarly termed. Sites, therefore, that appear as *castra* in early Frankish sources which were clearly Roman settlements can probably be added to this list of small walled Roman towns. Thus Carignan-Yvois yields many Roman finds and is called a *castrum* by Gregory (HF 4.18), as well as in many saints' lives, although the Roman walls have yet to be found. Naix-sur-Ornain has revealed many Gallo-Roman remains (Gilquin 1970), but no walls. Perhaps there were none, for although Fredegar (4.38) called the site *Nasio castro*, coins minted there were NASIO VICO.⁴

These late Roman military forts, these *castella*, are assumed by Böhner to have been royal centres of power and occasionally residence. The assumption is that military sites, as quasi-imperial property, were taken over by Merovingian kings as inheritors of the imperial fisc and as representatives of imperial rights and duties. He repeatedly suggests that the *Reihengräber* of such forts were those of the

4. The implications are probably greater for Romanists than medievalists. A list of late Roman walled towns in Gaul might be enlarged by systematically working through Merovingian sources. Thus I could offer the suggestion that Blaye was a late Roman fort – something almost impossible to test for it has subsequently been the site of every type of fortification down to this day – guarding the Gironde as one of the Saxon shore forts. As far as I am aware the suggestion has never been made.

'king's men' who administered the royal property and does so sufficiently often that others repeat his words almost verbatim for other *castella* (e.g. Precht 1980, 189 of Deutz). Such an interpretation of the *Reihengräber* is, to say the least, fanciful. The theme of royal Merovingian adoption of the imperial fisc will be discussed in the next chapter, but it should be noted here that only Andernach yields positive evidence of a Merovingian palace, while a comital seat appears to have been moved from Trier to Bitburg in Carolingian times and a Carolingian royal villa is found at Rottweil five centuries after the Romans abandoned the *agri decumates*. Furthermore, while palaces may be inferred to have existed in towns from reported royal stays there, it is a rarity to find Merovingian kings in such small walled towns as Zülrich. The relative unimportance of these bishopless towns makes it unlikely that they attracted much royal interest.

The power of Böhner's claims is such that Alzey, Andernach, Bad Kreuznach, Bitburg, Bonn, Deutz, Neumagen, Neuss, and Zülrich all figure in the text of Brachmann's (1983, 60 ff.) thesis under the heading of 'the Franks' connections with late Roman fortifications' and many other late Roman *castella* feature in an appendix. Towns, on the other hand, receive little more than a passing mention. No explanation is offered for the discrepancy. The emphasis on late Roman *castella*, it would appear, is because they were sufficiently small to be considered 'private' in a way that towns could not. In fact, when Brachmann does draw towns into the discussion, it is to suggest that the Merovingian kings effectively adopted them as enormously spacious, but virtually empty palace sites. The *praetoria* or *palatia* inside the town was considered a particularly suitable residence because of the defence offered by the town walls. Thus the interpretation of empty towns inhabited only by a king and bishop alone is adopted by Marxist as well as bourgeois historians.

These late Roman frontier forts along with other Roman walled towns lacking a bishop were termed *castra* in the Merovingian period. I (1987) have argued elsewhere that Gregory of Tours used the term to mean two other types of sites. One was simply 'a camp', and the terminological use, such as *castra ponere*, was classical. It appears frequently in descriptions of military campaigns, but, I have argued, the camps were not necessarily fortified and thus not to be understood as a Roman marching camp. In this respect it is interesting to note that while *castrum* appears only three times in the Vulgate bible, *castra* in the sense of camp appears over three hundred times. Carolingian glossaries regularly translate *castra* as *herberga* 'shelter' or 'lodging', a word otherwise used in glossaries to translate *statio* or *tabernaculum* (Köbler 1972). Thus, again, no sense of fortification is found.

The last category is little more than an extension of the walled Roman town,

namely a town or village that might otherwise be termed *vicus* but for a marked defensive nature, artificial or natural, and often of prehistoric ramparts. This latter group is best represented by the description Gregory gives of Chastel-Marlhac, a site easily recognised today (HF 3.13), to which we could also add Alesia:

The place (Chastel-Marlhac) was a natural fortress (*natura munitus*), for it was surrounded not by man-made walls (*ab exciso vallatur lapides, non muro-rum structione*), but by cliffs which rose sheer for a hundred feet or more. In the middle there was a great pool of excellent drinking water, and elsewhere there were springs which never seemed to fail, so that a river of fresh water ran through the whole place. This *munitio* was so extensive that the inhabitants farmed land and reaped an abundant harvest inside their walls.

Chastel-Marlhac (Cantal). A roughly circular natural plateau of basalt covering 40 ha, of which the north, west, and south sides all end abruptly in a sheer rock face. No signs of ramparts along the edge are visible. Gallo-Roman material allegedly found consisted of coins and *tegulae*. The -ac ending of the place-name is suggestive of a Roman personal name. Merovingian occupation is certainly attested by Gregory of Tours and a large central depression even today is nearly perennially wet, matching Gregory's description, as do the cliffs.

Bibliography: Fournier 1962, Salin 1950-59, Viollet-le-Duc 1867-89.

Alise-Sainte-Reine (Cote d'Or). Mont Auxois, Alesia, the famous site of Caesar's defeat of Vercengetorix, is a huge plateau covering 90 ha, revealing traces of drystone walling of *muris gallicis* and 'Priest' type around the rim of the plateau. Although it yields no good dating evidence it is believed to belong to the period of the great battle. It became a prosperous settlement in Gallo-Roman times, and was even supplied with a theatre. When the settlement fell out of use is difficult to say, but the coin sequence ends by the end of the fourth century; a cemetery and a building, 17 x 9 m., undoubtedly a church, overlie the Gallo-Roman level. To the south side this building was flanked by a 17 x 18 m. courtyard, in which burials in sarcophagi abounded. The graves were without grave-goods, although one seventh-century belt-buckle was found. No excavated evidence of Merovingian buildings exists, but there is plentiful circumstantial evidence of settlement. In addition to the cemetery, coins were minted bearing the legend ALISIA CAS and the territorial name *pagus alesiensis*, now pays d'Auxois, taken from the site implies continued importance into the early medieval period. In the *Life of St. Germanus, Bishop*

of Auxerre, composed ca. 480, Germanus visited a priest, Senator, noble by birth, at his *mansio* in *alesiensi loco*. In the *Life of St. Amatre, Bishop of Auxerre*, composed ca. 575 but dealing with a period nearly two centuries earlier, Amatre cured a man of noble family from *oppido alisiensi*. Another sixth century source calls Alesia simply *locus*. By the late ninth or early tenth century, the *Martyrology of Adon* describes Alesia as *olim fortissima civitas, sed a Iulio Cesare fuerat destructa* implying that the settlement was now very small if not abandoned. Its decline had probably begun earlier for early in the eighth century the monastery dedicated to Regina was transferred to the monastery of Flavigny and Regina's remains were similarly translated.

Bibliography: Fournier 1978, Joffroy 1960, Le Gall 1963, Marilier 1973, Vieillard-Troiekouroff 1978.

Chastel-Marlhac was clearly at the opposite end of the spectrum from Dijon. It may be significant that no count or leader of the people of Chastel-Marlhac is mentioned by Gregory and that the total ransom for the fifty men captured by King Theudebert's men was a mere sixteen and two-thirds gold pieces. There was clearly no one of any rank at all amongst these men, for elsewhere we read of Bishop Aetherius rescuing his licentious priest from execution by the payment of twenty gold pieces, and the slave Leo being sold as a cook for twelve gold pieces (HF 6.36, 3.15). Cabaret, Cabrières, and Dio share in common with Chastel-Marlhac the characteristic of being only tiny communities today although this need not imply they were always so small, for even these small *castra* appear to have been at least village-sized. Chastel-Marlhac could not have had less than a couple of hundred inhabitants to have had fifty men captured and yet remain sufficiently protected, to escape pillaging, and pillaging the Auvergne was the expressed intention of Theudebert's campaign. Alise-Sainte-Reine was no doubt larger. Unlike the Roman *castra* discussed above, Alesia would probably have been considered a *vicus* by the Romans, being designated *castrum* in the sixth and seventh centuries on the basis of its prehistoric ramparts. That it remained a large village or small town is suggested by the use of its name to designate the *pagus* and by the anecdotal references to noble families with *mansiones* there. Regina, too, having founded a nunnery there, was surely of a noble family.

That *castra* were for the most part villages or small towns appears inescapable. Discussing the *Notitia Galliarum*, Rivet (1976) suggests that

by the late fifth century it [*castrum*] had come to be used not merely of a military base but of any town below the rank of a *civitas*. . . . *castrum* and *castellum*, therefore, both seem to have begun to supplant *vicus*, with

castrum tending to usurp the legal meaning (approximately, 'sub-ordinate town') and *castellum* the colloquial.

In fact *castrum* was far from supplanting *vicus* in Merovingian vocabulary, for *vici* appear more than twice as often in the works of Gregory of Tours, while on the legends of Merovingian coins some 40 *castra* appear alongside some 175 different *vici*. The change of many sites' qualifications from *vicus* to *castrum* can be accepted. Johnson (1975) recognises the change of Yverdon from *vicus* to *castrum*, and reasonably argues that the same thing occurred among a number of Rhineland settlements: Bitburg, Junkerath, Arlon, Neumagen, Pachten, and Alzey. In the second century all were called *vici*, in the fourth century or later, all were probably called *castra*. The change in name coincided with the provision of walls. Such a change is similar to the change from *castra* to *civitates* of those sites that later acquired bishops.

Gregory himself used both terms, *castrum* and *vicus*, for each of the two sites, Chinon and Loches. Comparisons between different sources similarly reveal occasional interchangeability. This is so for Nieuil-les-Saintes and Dio. Amboise was a *vicus* to Gregory but appears as a *castrum* in the *Dialogues* of Severus. Naix-sur-Ornain is a *castrum* in Fredegar (4.38), but appears as NASIO VICO on coin legends.

While different authors might differ in their names for settlements, as we might vary between town and village, it is striking how common the same term is used to describe each settlement. Coin legends, alas, frequently make no distinction. Thus EPOCIO, SAREBURGO, and SCARPONNA, are examples drawn from Belgica Secunda. The latter two, Sarrebourg and Scarponne, both had late Roman walls. Sarrebourg's name, with a *burg* ending, suggests it was probably seen as a *castrum* and a charter of 715 actually calls it *castrum Saraburgum*. Epocium, or Yvois-Carignan today, was called *castrum* by Gregory (HF 8.15) and appears in saints' lives as *castrum* or *oppidum*. A thorough search through all the documentary sources is likely to reveal a generally consistent use of terms for individual sites. We can find consistency of naming between different textual sources and coins: both Châteaumeillant and Chinon appear on coins as well as in the pages of Gregory as *castra*. Blaye occurs as a *castrum* in the work of the continuator of Fredegar. Although the scribe might have been influenced by Gregory's use of the term in a hagiographic work (GC 45), its appearance in Bishop Bertram's testament likewise as *castrum* makes the consistency almost indisputable. Osoppo in the Italian Alps was called *castrum Osopus* as Venantius Fortunatus journeyed past it (V. S. Martini) as well as by Paul the Deacon when he recorded its attack by Avars (*Historia Langobardorum* 4.37). Another interesting case is that of Utrecht which, once a Roman fort, reappears much later in

historical sources related to Willibrord's and Boniface's missionary activity, where it is almost exclusively described as *castrum*. Finally, we can note Auguste Longnon's observation that 'château' still forms today an integral part of the name of five of the *castra* mentioned by Gregory: Chantoceaux, Chastel-Marlhac, Châteaudun, Châteaumeillant, and Grèzes-le-Château.

It is often said that sites deemed to be *vici* by Merovingian definition never appear to have been fortified. As far as I am aware, no towns with any evidence of late Roman walls ever figured as *vici* in the writings of Gregory of Tours, Fredegar, or on coin legends. Although no *vicus* is described in detail by Gregory, they never appear in any context to imply fortification. Isidore of Seville thought the lack of fortifications to be the etymological derivation of *vicus*, and although his etymology was seldom sound, his method involved making plausible associations (*Etymologiarum libri* 15.2).

Gabriel Fournier (1962, 195) suggests that *vici* were not necessarily unfortified, because Chinon, Loches, and Amboise are also mentioned as *castra*. This approach, however, mistakenly mixes terms and attributes. Some *vici* were fortified and others not, but small towns and villages, if 'fortified' were called *castra* and undefended, *vici*. The distinguishing characteristic of *castra* was their perceived defensive nature. It is interesting, therefore, that Köbler (1972) argues the same thing for *burg* before the eleventh century, suggesting that in modern geographers' terms the sites termed *burg* can scarcely be taken as a unity. His interest is in vernacular terminology, but his results may be projected back on to the early medieval Latin terms. Thus *vicus* is never glossed as *burg*. Like *burg*, the term *castrum* was applied because of a perceived defensive quality, but that the 'defences' could be ambivalent is not surprising. While stone walls are mentioned or have been found at Dijon, Vitry-le-Brûlé, Tournus, Melun, and Beaune, the defences of Chastel-Marlhac and *Vabrense castrum* were described by Gregory of Tours expressly as being due more to nature than human art. At *Vabrense castrum* and Luynes Gregory claimed that the works were ancient (*antiquitus*), as Severus did of Amboise, producing in our minds the image of something like Iron Age ramparts. The *-dun* endings of Melun, Châteaudun, and the unlocated *Tauredunum* – which one should note was located 'high on a hill' – reinforce this image. At Amboise and Alise-Ste-Reine the prehistoric ramparts are still visible. Clearly *castrum* does not allow neat categorisation.

Amboise (Indre-et-Loire). The plateau above the confluence of the Amasse and Loire, called 'les Chatelliers' has been the focus of repeated settlement in prehistory. Roughly triangular in shape, the apex is totally obscured by a

medieval castle, which lies behind an earthen rampart and ditch, some 150 m. in length. Having rounded ends, the rampart does not look as if it ever stretched to the edges of the plateau and may well be a later work connected with the triangular outwork between it and the castle. Finally, the plateau is protected by a rampart and ditch some 450 m. in length, which bars this natural spur on its weakest side. The rampart attains 3 to 4 m. in height and some 14 m. in width, terminating at both ends in a small mound slightly higher than the rampart, although one end is obscured by a farm, La Motte. The difference in height from rampart to ditch bottom can reach 6 m. Although traditionally attributed to the Romans, the only possible Roman remains found are pieces of brick or tile rims. Severus mentions Amboise in his *Dialogues* when he mentions monks being installed here in an old *castellum* (*castello illo veteri*).

Bibliography: Fournier 1978, Millotte and Riquet 1960, Vieillard-Troiekouroff 1976.

It is easy to see now why Gregory oscillated between *castrum* and *vicus* for Amboise. The defensive nature *castra* might have had was often more visual than practical.

Many towns that are the descendants of settlements termed *castrum* in Merovingian texts now have medieval castles perched on a dominant point dominating the town. While it is tempting to see this as the site of the Merovingian *castrum* in general it is rare to find such coincidence, for the earlier *castrum* generally covered a much larger area. Thus the site chosen for one was often inappropriate for the other. This can be seen most clearly in a number of sites in the Auvergne, including those mentioned by Gregory as *castra* and sites of similar morphology yielding fifth-sixth century paleochristian ware. Much of our knowledge is due to the efforts of Gabriel Fournier, but it is quite probable that the Auvergne, because of its mountainous nature, lent itself to the development of such sites. Besides Chastel-Marlhac, two other sites were mentioned by Gregory, Thiers and Vollore. The remainder listed below are analogous.

Thiers (Puy-de-Dome). Situated at the entrance of the valley of the Durolle, the upper part of the town occupies a north-south spur dominating the valley by about 80 m. There is here a church dedicated to St. Genes and another church at the extremity of the spur dedicated to St. John the Baptist, while below by the river bank an ancient monastery contained a church dedicated to St. Symphorien of Autun. Gregory recounts how

Theuderic's army set fire to this *castrum* with the result that the wooden church caught fire from the burning houses, although the relics, stones supposedly splattered with the blood of the martyr St. Symphorien, miraculously survived (GM 51). Gregory also recounts how a church dedicated to St. Genes was built at a spot which lay adjacent to the *castellum* by a path which led to a forest. The implication is clearly that the *castrum* did not in fact lie on the spur but by the bank of the Durolle, where artificial defences would have been necessary for qualification as a *castrum*.

Bibliography: Fournier 1962, Fournier and Fournier 1958, Vieillard-Troiekouroff 1976.

Vollore (Puy-de-Dome). Set by a Roman road leading from Lyon to the Atlantic are the remains of a thirteenth century castle at the extremity of a complicated spur from a ridge of granite hills. The accompanying settlement has now disappeared, while the present centre concentrates around a church dedicated to St. Maurice, a popular dedication of the fourth and fifth centuries. Gregory of Tours names Vollore *castrum* on three occasions and *castellum* once. Like Chastel-Marlhac and Thiers it was the site of a siege by King Theuderic. It was captured by the breaking of the town walls (*irruptis Lovolautrensis castri muris*) (VP 4.2).

Bibliography: Fournier 1962, Vieillard-Troiekouroff 1976.

Chastel-sur-Murat (Cantal). On the summit of a spectacularly isolated basaltic hill surrounded entirely by steep sides, some precipitous, is an ancient parish church dedicated to St. Antoine. The 1-2 ha. summit forms no flat plateau, but areas of uneven height dominated by a central boss. A recognisable edge to the plateau exists and reveals a single course of some five stones by the entrance path along the northern edge, while the southern edge reveals an unnaturally up-turned grassy bank 'lip', perhaps the remains of a wall. Excavation from the beginning of the century revealed numerous finds of the late Iron Age, Roman period and produced many pieces of paleochristian ware, now housed in the Aurignac museum.

Bibliography: Delort 1901, Fournier 1962, Vire and Guebhard 1908, 1910.

Saint-Flour (Cantal). The old part of town, set on a rocky plateau with sheer north, east, and south faces, and a gentle approach from the west, revealed, in excavation early this century, coins of the first century BC, Gallo-Roman

vestiges, and a fragment of a large paleochristian black plate. The church here in Carolingian times was gifted to Cluny.

Bibliography: Boudet 1910, Delort 1901, Fournier 1962.

Saint-Floret (Issoire). Overlooking the modern village is a plateau with steep sides all around, called 'le Chastel'. Iron Age pottery is abundant, while some Roman and paleochristian pottery has been found. Today the plateau only supports a church.

Bibliography: Fournier 1962, Fournier and Fournier 1943.

Ronzières (Issoire) (Fig. 2.14). A basaltic plateau of elliptical shape, some 180 x 80 m., covering 1 1/2 ha. with an abrupt face on the south side, and steep slopes around the remaining sides, although of lesser height than most of the other sites mentioned above in the Auvergne, is presently occupied by a church dedicated to Our Lady and Saint Beauzaire. A church in the village below once dedicated to St. John the Baptist suggests that the pair were once probably a Merovingian baptismal group. The church is situated at the southern edge of the plateau, separated from the slightly higher northern section by an earthen bank, now covered by a hedge. Excavation has been undertaken on a very small scale by Fournier from 1964 to 1971. The rampart was of earth with a revetment of drystone walling on the northern face, the southern face may have had similar treatment although disturbed by house foundations of presumed 11-12th century date. Although not fully excavated, a ditch may have protected the northern side for a great depth of soil was found here, although elsewhere to the north it seldom exceeds 10 to 20 cm. Some two dozen graves were found in the course of excavation, some under the houses, and some apparently under the rampart. They were without grave goods, oriented west-east in stone-lined and covered graves. It seems generally true of the Auvergne that early medieval graves were unaccompanied by grave-goods, making dating of these difficult. Within the rampart were found fragments of paleochristian ware, providing a *terminus post quem*. Two areas were examined in the northern section of the plateau. The eastern area revealed a small room, 3 x 4 m., with walls still standing some 3 m. dug into the soil/rock. *Tegulae* were found, probably once covering the roof, and some paleochristian ware in association. An area just north of the rampart produced large quantities of paleochristian ware, and a stone wall running parallel to the plateau edge with two perpendicular walls running from the wall into the

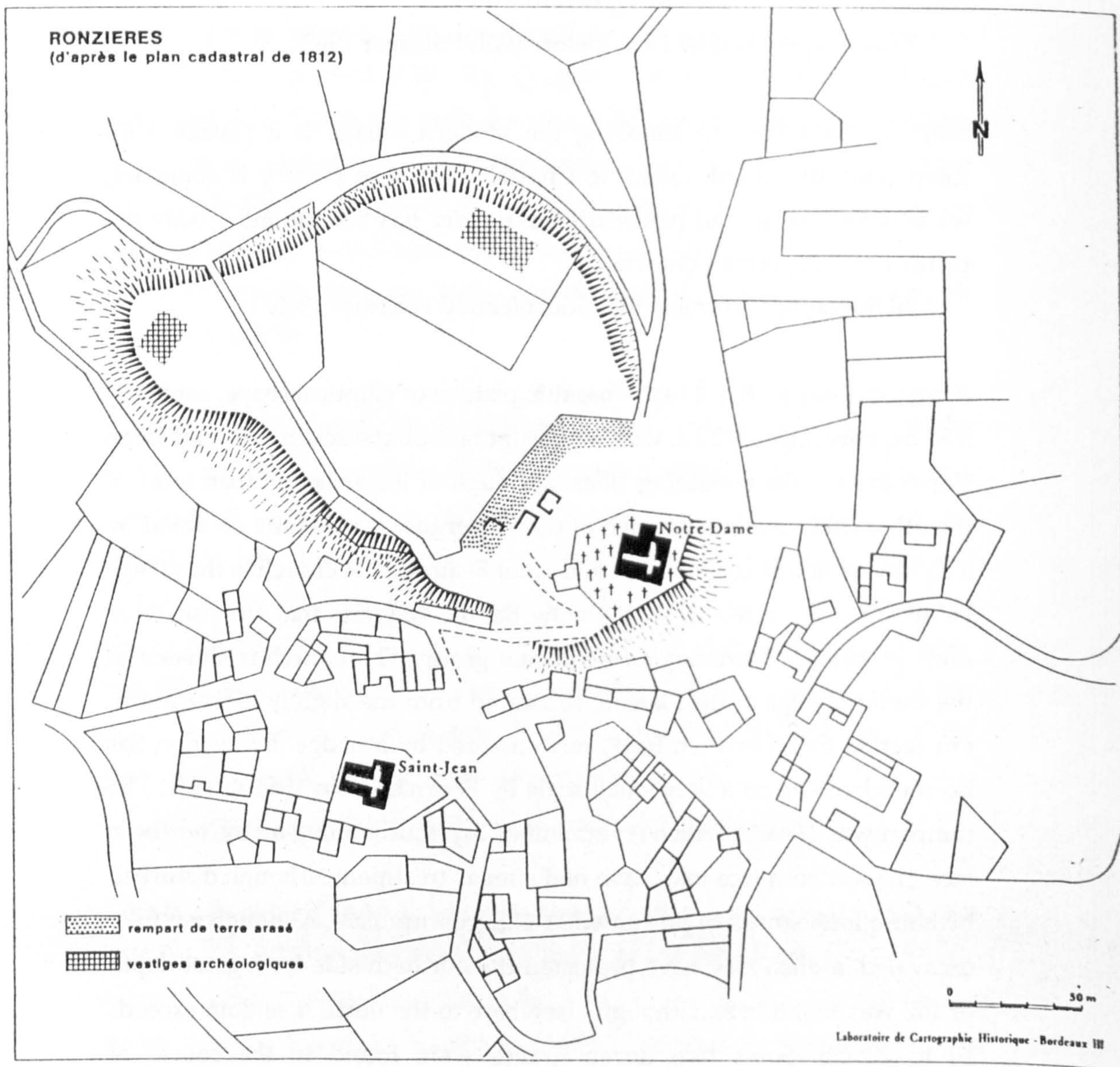


Fig. 2.14 Ronzières, an enclosed Merovingian settlement (after G. Fournier).

plateau. It has been interpreted as relating to domestic settlement. The rampart wall is seen as post-dating this settlement, with earth containing paleochristian ware taken from here used in its construction.

Bibliography: Fournier 1962, 1978, ca. 1980

The suggestion has been made that Chastel-sur-Murat, St. Flour, St. Floret, and Ronzières, although not mentioned by contemporaries in the surviving literature, would indeed have been reckoned as *castra* or *castella* by the

Merovingians. The existence of 'chastel' in the toponymy of Chastel-sur-Murat and St. Floret support such a contention, as do their topographical situations. If accepted, we can suggest some of the following characteristics of *castra* in the Auvergne. First, they were situated on naturally defensive plateaux, ideally surrounded by abrupt sides. Second, the plateau surface was often quite extensive in area (1-3 ha.). Chastel-Marlhac is exceptional in its peculiar geological setting. Third, previous Gallo-Roman occupation was generally the rule. Fourth, the existence of a church in Merovingian times seems very likely in most cases.

Such sites can hardly be interpreted as temporary refuges. The existence of a church would suggest continual rather than intermittent use. Similarly the discovery of paleochristian pottery on these sites, but seemingly not in the successor sites often at the foot of these hills (where modern activity is so much more intensive that the chances of recovery should be much higher), must mean that the settlements using and discarding the pottery were on these plateaux. Direct evidence of occupation is clearly limited as excavation is so rare. Ronzières, the only site excavated by modern techniques, has yielded evidence of one well-built stone dwelling and a pottery scatter found associated with stone walls, which at the very least suggests that occupation rather than manuring was the mechanism for their deposition. The frequency with which Gallo-Roman material is found on these sites reinforces the idea of settled communities, implying continuity from the Roman period, if not indeed from the Iron Age. It also explains why large areas were involved: these were sites of permanent occupation, not simply occupied for days during periods of danger. Chastel-Marlhac was no remote fastness with its cultivated fields. These were the local fields of the settlement, which no doubt underlies the modern hamlet.

In the late fifth century *castella* or *burgi* in the mountains are mentioned by Sidonius Apollinaris (*Carmen* 2; *Epist.* 4.15.3; 5.14.1) as places to which the inhabitants of the Auvergne fled in times of danger. Thus when Theuderic attacked Chastel-Marlhac, Thiers, and Vodable, it seems more than likely that the 'inhabitants' included a large number of people from neighbouring settlements. The evidence that has just been discussed makes it quite impossible to argue that these *castella* were merely *Fluchtburgen*, especially built hillforts or castles only occupied in times of danger. Sidonius is, however, frequently cited as evidence for just such a proposition (e.g. Gilles 1985, 72).

It is not impossible that some of these sites were effectively villas. Thiers and Vodable may have been too large and thus might be seen as *castra* in the sense of fortified *vici*. Chastel-Marlhac, Ronzières, and St. Floret, on the other hand, must

have been rather smaller agricultural settlements. Although others have not considered the possibility, it is almost certain that large prosperous villas, like Sidonius's own Avitiacum, must have had appurtenant properties in the hills for use in transhumance. Thus while neither Sidonius nor members of his class would have thought of living anywhere but on their great villas or in Clermont, when life and limb was threatened, they were not above fleeing to join their herds in dependent settlements perched on basaltic plateaux deeper in the Massif. That they may have been summer retreats or convenient estate properties from which to go hunting should also be considered. The existence of a small church on these remote settlements would thus be easier to understand.

Thus one can argue that in only in a very few instances *castra* were neither walled or otherwise defensible towns or villages nor prehistoric hillforts, whatever purpose they may have been put to in post-Roman times. The special case of mountain settlements in the Auvergne may well have been parts of lordly estates, but there is certainly no evidence that they were ever purpose-built refuges in the fastness. This is almost the opposite of what is commonly argued for a number of sites in the Eifel, Hunsrück and Ardennes. These sites are conveniently summarised by Johnson (1983). They were largely a late Roman phenomenon, and will only be discussed briefly here, for although continuity is clearly recognisable, in some regions it was less marked than in others, and the sites as a whole appear to have been uncommon and minor. The discussion is, nevertheless, essential because they have contributed enormously to the belief in *Fluchtburgen* and acceptance of the existence of such sites has contributed to some odd conclusions about Frankish villas and palaces.

Late Roman Irregular 'Forts'

Behind the Roman *limes* there are three areas where rural 'hill-top defences' are found to be concentrated: around Trier in the Hunsrück, Eifel, and Ardennes, typically along river valleys like the Moselle, Saar, Semois; along the northern Alpine foothills, in northern Switzerland and southern Bavaria; and in the Carnian and Julian Alps of southern Austria along the Drau and Gail and north-eastern Yugoslavia.

On the southern side of the Alps, particularly full excavation has taken place at Invillino (Bierbrauer 1987). Bierbrauer (1985) places it within its broader historical context, particularly with reference to Honorius and Constantus III's attempts to protect northern Italy early in the fifth century, the *tractus Italiae circa Alpes*. It is supposed that garrisons were effectively militia bands. The Ostrogoth Theodoric the Great sent out several letters through his secretary Cassiodorus

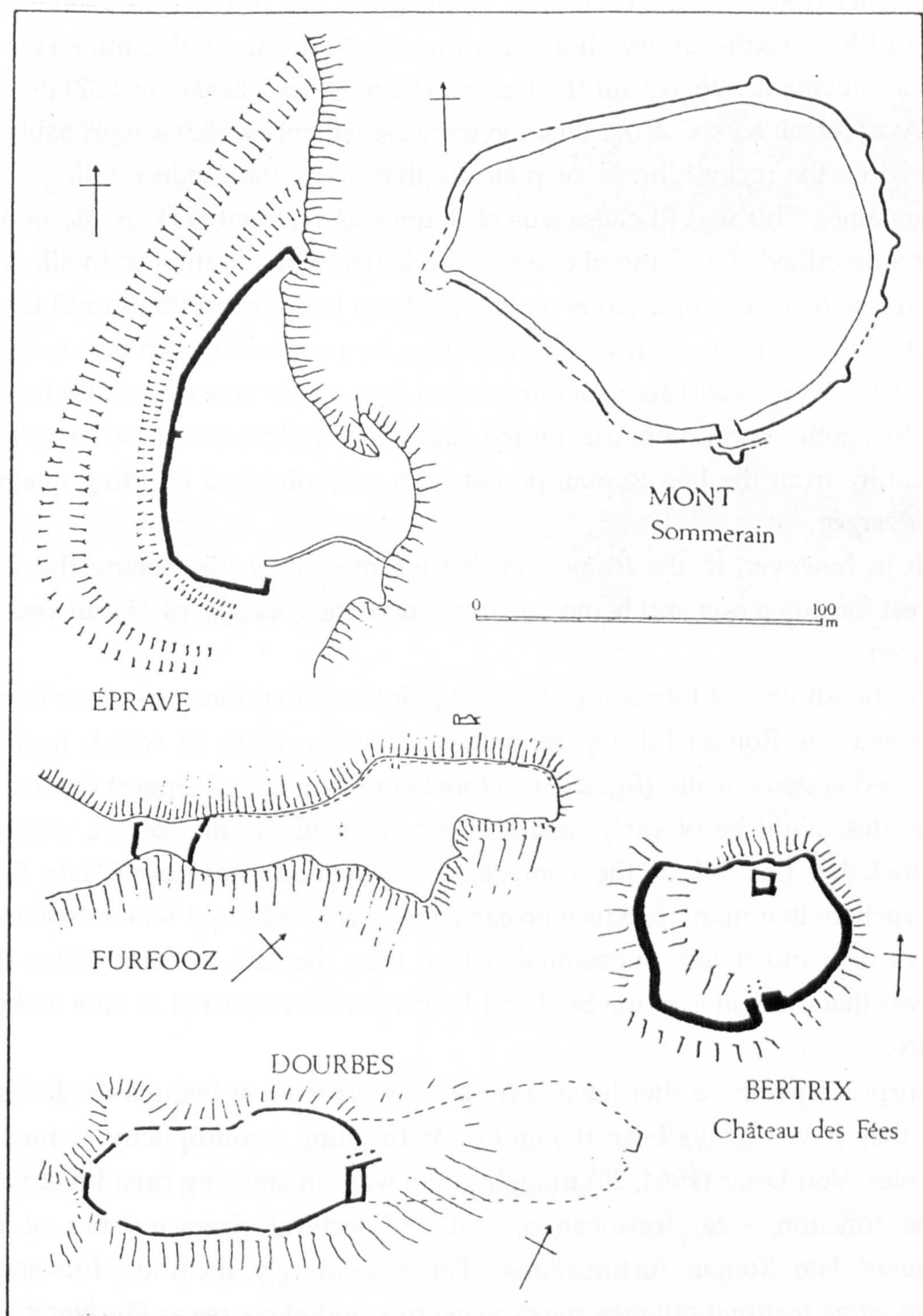


Fig. 2.15 Late Roman irregular 'forts': Bertrix, Dourbes, Éprave, Furfooz, and Sommerain à Mont (after Johnson).

(e.g. *Variae* 2.5, 3.48) demanding that walls be maintained and *castra* built. Just outside Trent, all Goths and Romans were to help in the building at the *castrum* Verucca. This *castrum*, now Doss Trento, is naturally protected by rocky cliffs. As Bierbrauer (1985, 497) notes, it is no more correct to call this a *Refugium* with its large fifth- to sixth-century church than to call it an Ostrogothic military post. It was a settlement. When Paul the Deacon (*Historia Langobardorum* 4.37) described the Avar attack on six *castra*, they too were settlements which are probably to be located on the rocky hillocks or plateaux that lie by the modern villages of the same names. This was likewise true of Reunia; as Fortunatus (*V. S. Martini*) rode by, he described it as lying above the Tagliamento. Excavation at Invillo, Paul's Ibligo, has revealed continuous occupation from late Roman times until the mid- or later seventh century. It would, therefore, be particularly perverse to think of these sites, which yield some of our best evidence of profane stone built houses of the Ostrogothic and Lombardic period, and which yield some of the best signs for continuity from the late Roman period with undiminished building quality, as *Fluchtburgen*.

It is, however, to the Rhineland that we must now turn, being the area of interest for this thesis and being the area where the concepts of *Fluchtburgen* first surfaced.

In the Rhineland Johnson (1983, 227) points out that there are some hundred suspected late Roman hill-top refuges, less than a dozen of which have been examined systematically (fig. 2.15). Before being tempted to suggest that many of these sites might be of early medieval date as well as late Roman, we should acknowledge that all of the competently excavated sites reveal late Roman construction, that many produce no early medieval finds, and when they do, it is always of a much less substantial nature than the late Roman. Gilles (1985) believes that most sites in the Eifel and Hunsrück are restricted in date to the late empire.

Surprisingly, these sites have never been interpreted as being a single distinct type; they have always been thought of as fulfilling a multiplicity of functions and roles. Von Uslar (1964, 20) thought there was 'an amazing breadth of variety in the function, size, fortification, and architectural characteristics of these 'irregular' late Roman fortifications.' These seemingly included functions as *castella*, *burgi*, fortified villages, garrison centres, and of course as *Fluchtorte*. Gilles (1985, 13-5) reviews some of the lines of thinking common in German scholarship since the middle of last century, when the idea was that all the fortified sites of the frontier region were built on imperial orders. These hillforts were built as refuges for the local population. Late last century there was a move to see these

sites as part of the road defence system, like the small look-out posts, *burgi*, elsewhere. In post-war years the tendency has been to return to seeing them as refuges for the local population, but as the result of individual initiative rather than central organisation. A more marked tendency, and one which Gilles typifies, is the adoption of all of the above possibilities in greater or lesser degree, although applied to different sites and not to the group as a whole.

Studying the region of the Treveri, Wightman (1967) recognised two types of fortification in addition to the *castra* and *burgi*. Both were characterised by their siting on hill-tops but were distinguished from each other by their distance or proximity to Roman roads. The latter included Furfooz, Polch, Katzenberg, Williers, Herapel, and Volklingen, and were interpreted as being part of the military defensive system and associated with occupation by *laeti* or *foederati*, the former were presumably occupied by civilians (the functional division favoured by Mertens and Johnson discussed below), although whether temporarily or permanently settled Wightman does not say. This postulation has been supposedly further strengthened by the discovery of chip-carved artefacts at Furfooz, Volkingen, Eprave, Ben Ahin, Dourbes, and more sparsely at Krembach, Polch, Herapel and Williers, and the interpretation of them as the equipment of *laeti*. Furfooz (fig. 2.15) is the type site with its associated cemetery containing predominantly male burials, most with weapons. Even if the attribution of late Roman chip-carved metal-work to *laeti* or *foederati* were tenable, which has been doubted (C. Hills 1979), a distinction between military and secular occupation would be impossible, for the *laeti* and *foederati* performed military service in return for their settled life within the Empire.

Moreover there are difficulties for a military interpretation. One is to explain the great variety in constructional techniques. Johnson summarises the range of types (1982: 230):

the natural topography was normally assisted by defences sufficient to provide adequate protection. This might mean, therefore, either a stone wall, mortared or drystone, or an earthen bank. Where the hill's natural defences made approach difficult, it was sufficient to provide a protected enclosure by a simple barrier wall across a narrow portion of the hill. Otherwise, the whole hill-top might need encircling with defences.

Such a wide variation speaks against central organisation and suggests a local response. Gilles (1985), on the contrary, suggests that the frequently found tiles should be seen as military products. This is scarcely convincing on its own, but some sites, like the Niederburg bei Kobern-Gondorf yield tiles stamped L(egionis) XXII CV. He also suggests this was the site of the Merovingian mint CONTROVA CASTRO, although there is effectively no real evidence to confirm the

hypothesis. An explanation for the 'irregularity' of the construction is offered in the form of an inscription from a sarcophagus revealing that the deceased had held a special commission to combat highway robbery. Thus unlike the typical military *castella* and *burgi*, these irregular hillforts were what we might call posts for an 'interstate police'. That the Eifel-Hunsrück was an area where highwaymen would readily find security is plausible, that there was a special network of military forts in the sparsely inhabited hills is implausible. Indeed, one weakness of almost all the proposed explanations is that they fail to account for this distribution in hilly, thinly settled areas. Too often it is assumed that the distribution is primarily that of the empire's borders, but such cannot be said of the Ardennes and such sites are lacking in more open and heavily populated areas by the frontier.

Many hill-top fortifications are sited away from Roman roads and 'deep in the heart of the countryside' (Johnson 1983, 277). If these can be interpreted as non-military, is it necessary to create a division of functions for these hilltop sites as a whole, which clearly have so much in common? If the sites sited so far from Roman roads can be interpreted as 'civilian', would it not be a credible alternative to envisage all these sites as essentially non-military? That is not to say that military forces might not have been billeted occasionally at these sites, or that they could have fulfilled roles as staging posts when required.

The use of these sites as refuges makes even less sense. Johnson (1983, 231) interprets four recently excavated Belgian sites (Bertrix, Cheslain d'Ortho, Roche à Lomme, and Sommerain à Mont) as having been predominantly 'temporary refuges for men, beasts, or produce in the face of barbarian threat or invasion.' Essentially they are to be understood as 'safes'. He is, however, merely reflecting the prevailing opinion of Mertens and Brulet (1974, 49). I suggest that this 'refuge' interpretation is untenable for several reasons. For Mertens (1960, 73) the '*faible densité de trouvailles*' provided an indication that Kaarlsbiurg was not permanently occupied and probably served as a refuge. Leslie Alcock (1968, 83) suggests that the quantity of ceramic refuse is no safe index to the social status of a site, when discussing the paucity of material from Degannwy. Greater cleanliness might simply reflect greater sophistication; cleanliness might explain the 'meagre density of finds'. At any rate, the 55 square metres of excavated area at Kaarlsbiurg (less than one percent of the site), concentrated on the ramparts, is clearly insufficient to allow conclusions about the permanency of occupation. Few excavations are anything like extensive enough to provide adequate negative evidence of permanent occupation, without which permanent occupation seems a more reasonable assumption. Even at the tiny Château des Fées at Bertrix only

some fifth of the interior was investigated, revealing only one building, a possible watch-tower (Matthys and Hossey 1973). Johnson notes (1983, 231) that 'at a number of sites, the only building discovered to be of late Roman date is a small square tower . . .' but these were perhaps visible on the surface before excavation, as at Bertrix, or were on the highest point within the fortification, as at Bertrix and Katzenberg. This is precisely the most commonly sampled area within a fortified site after the gateway so that their discovery was not hazardous, and the idea that a single look-out tower was the only building present cannot be maintained. At Bertrix considerable quantities of Roman pottery and tile were found. The tile can surely only imply buildings and occupation. Without doubt, at Bertrix and other sites, buildings must have been missed. At Cheslain d'Ortho, *contra* Johnson's claim (1983, 228) that 'no buildings within the defended area have been identified', three parallel rows of five post-holes all carefully cut, perfectly round and 'almost polished' smooth, form a rectangle 9.4 x 4.3 m. (Mertens and Remy 1971). This is undeniably a timber aisled-hall. The associated finds date it to the fourth or early fifth century. Another of the Belgian sites, Dourbes, is accepted by Johnson as yielding evidence of long and 'relatively constant occupation' because of the long chronological span and wealth of finds, although here no traces of buildings were positively identified. At Furfooz permanent occupation is similarly accepted although there is no evidence of dwellings within the fortification walls. This is not surprising, for, excavated in 1932, 1955-8, and 1974-6, only five or six very thin trenches explored the interior and all dated to the 1932 excavation. The evidence of a bath-house and cemetery could be viewed as conclusive evidence of permanent occupation and should be a warning against arguing from negative evidence of structures within other partially excavated sites.

These so-called 'late Roman irregular forts' might better be interpreted as *castra* in the Merovingian sense of villages, *vici*, which happened to be enclosed behind a wall or built on a hilltop or plateau that was not easily accessible. The smaller sites might even be the shape of villas-to-come, or appurtenances to villas. Because they are found primarily in less fertile, hilly, thinly populated areas, we might think that they formed the settlements of a population less socially and economically developed than in the fertile villa-covered basins and river valleys of northern Roman Gaul. In short, these more 'backward' areas might have had more in common with settlements beyond the Roman frontier. They might be better compared with the likes of Runder Berg than with Roman *limes* forts (see chapter five). While it is quite plausible to suggest that the locally powerful might have lived in such a settlement in early medieval Hunsrück, the

potentiores majores of fourth-, fifth-, and sixth-century Gaul did not live in such backwaters. They continued to live, as their predecessors had, in villas.

Villas and Farms

This chapter began by mentioning the conclusion of Chapelot and Fossier that settlements were mobile and ephemeral until the ninth century, when they became fixed after which they grew into the nucleated medieval villages of the French countryside that are known to historians. Such an interpretation may be fitting of early medieval Danish settlements, as at Vorbasse, but how realistic is it of Merovingian settlements?

The shifting Danish settlement pattern is derived entirely from archaeological evidence, and Chapelot and Fossier rely heavily on archaeological evidence for the early medieval period in France, eschewing textual evidence. This brings with it several problems. The model of shifting settlements and population movements is wrongly used to explain some archaeological phenomena. A bizarre explanation of the distribution of sixth- and seventh-century artefacts in Germany is put forward (1985, 58): the whole north German plain, devoid of finds, is interpreted as having been abandoned in the fifth and sixth century by a population which migrated en masse to England and south-western Germany! The key to understanding the distribution map is, of course, that it reflects the *Reihengräber* burial practice. If Chapelot and Fossier's argument were taken to its logical conclusion, we would have to concede that there was a mass migration out of southern Gaul as well, particularly Provence, to the western seaboard and Gaul north of the Seine. Furthermore we would have to see all Gaul deserted in the Carolingian period as the population migrated into Saxony. Gaps in the archaeological record must be understood in terms of the depositional processes which create them. In this case the row-grave burial custom produced a highly visible archaeological record. The fifth century, when inhumed bodies were not clothed with durable artefacts like pins, brooches, belt-buckles, and the like, remains virtually invisible archaeologically.

Even if most Gallic settlements could be shown to move, like Vorbasse, every few hundred years, the mobility of the Danish settlements does not imply the ephemeral nature of occupation and underdeveloped agricultural practices that Chapelot and Fossier infer. However there is not even much archaeological evidence for such mobility. The possibility that Merovingian and early Carolingian rural settlements did not remain in the same location for long, but moved every few generations is a hypothesis worth testing, but if the textual evidence is examined, the hypothesis bears little scrutiny.

If the rural settlements of Gaul formed a shifting kaleidoscope until the ninth and tenth centuries, we could well imagine that small rural hamlets mentioned in the sixth or seventh century would be impossible to find today. But such is not the case. Longnon's (1878) book is effectively a 'geography' of the places mentioned by Gregory of Tours. Margaret Weidemann (1986) has little difficulty in placing the hundred and twenty places owned by and disposed of by Bishop Bertram in his will of 27 March 616. This included not only villages and villas, but even places termed '*locus*' or '*locellum*'. Naturally not all named settlements can always be found and mistakes are surely made, but the continuity of settlement names – and we have records of thousands of named Merovingian villas – is so marked that it can only mean the continuity of occupation, even if we have to see the physical location able to creep a few hundred metres every few centuries.

Given the stability of towns and villages from the time of Gregory of Tours, a degree of stability in the countryside is only to be expected, especially as it was on the back of rural settlements that urban sites rested. That is to say, a well-developed hierarchy, both social and settlement, presupposes well-developed rights over land and those who work it, and thus the existence of real estate, land as property. The continuity of settlements from the early Middle Ages to this day expresses the continuity of proprietary rights.

How then are we to envisage the occupation of the Gallic countryside? Do we picture vast tracts of forest with small clearings containing a few miserable sunken-pit houses? Do we see seas of open fields surrounding thriving village communities of free peasant families? Do we imagine the dilapidated ruins of Roman villas with added timber halls surrounded by the huts of dependent serfs and slaves? The questions are simultaneously physical and social: how nucleated or dispersed were farmsteads and what was the degree of dependency of most peasants on lords?

To be truthful the questions cannot be answered with anything approaching certainty. Fustel de Coulanges envisioned Merovingian Gaul as a fitted carpet of villa estates; there was no room for villages or independent peasants. Orthodox Marxists, following Engels, populate fifth-century Gaul with village communities of peasants who are not only free from dependency but also almost innocent of any knowledge of land as private property. Both camps draw upon their own sources. Fustel de Coulanges came to his conclusion almost exclusively through his study of charters, but these were largely charters of the Church, a major landowner and exploiter of peasants, recording donations made by kings or other great secular magnates. Indeed all the written sources have biases which will tend to skip over the independent peasants. The great Carolingian polyptychs

record what was owed to the estates; by definition those who were independent, owing nothing, were of no interest and not mentioned. This lack of evidence for independent peasants means that Marxists must rely on their own long-term analysis of historical changes from the late Roman empire to the Carolingian period. This postulates the collapse of large-scale, widespread peasant exploitation, which is the basis of 'civilisation', hence the darkness of the Dark Ages. The later (much later) evidence of communal peasant activity, particularly in the form of open field agriculture and use of common ground, is taken to be survivals from the good old lordless days.

A major failing of the Marxist approach, which was likewise long favoured by the non-Marxist Germanic or Whig school (Stenton was a proponent), is that it fails to recognise the communal organisation of medieval agriculture as a late development. It was the result of a much more intensive agriculture; the twelfth century was perhaps two or three times more populous than the fifth and sixth centuries. Nor can it be fully disassociated from the relationship these peasants had with their manorial lord (admittedly it was not lords who forced or encouraged this communal organisation).

A major failing of the non-Marxist approach, however, is to eschew the Marxist analytical tools for understanding social relations of dependency, or to fail to develop new ones. Typically, 'bourgeois' scholars will rely on the terms used in the sources. By all means we must investigate these terms, try to understand the legal, social, and economic rights and disabilities of *servi*, *liberti*, or *colonicae*. But we cannot write a narrative history analysing the changes of the conditions of dependent peasantry *through* these terms or of the *ingenui*, for example. Why? Because their historical meaning changed through time. The 'free' of the sixth century were not the 'free' of the tenth century, who were not the 'free' of the thirteenth century. There were free and unfree in thirteenth-century England, where there were no slaves. When there were slaves, in the early eleventh century, a much greater percentage of the population was also 'free'. The most useful method is to analyse peasant social conditions in terms of their obligations to lords, labour, renders, or rent, their rights to move or exchange the piece of land they farmed, and their various legal and social disabilities, including rights of marriage without consent or appeal to courts. The lack of modern analytical concepts to understand dependency and reliance on the terms employed by medieval scribes has led many astray.

One example of how this theoretical naivety creates problems is that of Theodore Rivers's (1975) argument that nothing distinguished the dependency of *servi* and *coloni* in the Bavarian laws of the Carolingian period, for both paid

render in kind and both provided labour services. The relative weight of each was, however, markedly different. *Servi* spent half or more of their life, according to the lawcode, working directly for their lords and rendered only a small amount of produce. *Coloni* rendered much more produce, but then they were able to, for they spent almost all their time working their 'own' land, providing only occasional labour, and of a set kind, not simply whatever the lord's bailiff had in mind for the day.

The importance of analysing who organised and directed labour, rather than the legal designation of names, comes out in Marc Bloch's (1975) discussion of the end of slavery. For him slavery effectively ended when slaves were given a piece of land and left to farm it. *Servi casati*, slaves supplied with a *casa*, cottage, are seen to have become serfs who worked their own piece of land but rendered produce and labour to their lords, despite the continued use of *servi* to designate slaves who might be bought and sold in the market.

Medieval scholars often use the language of lawyers trained in Roman law to discuss property, ownership, and *usus fructus*. Our own, modern, western, capitalist ethnocentric feeling is that ownership and property is complete and that with it comes full rights of use and disposal, even if this feeling is not the overt bourgeois plot to disguise the historical nature of its development as Marxists long held. In the Middle Ages it is difficult to talk of ownership for there were layers upon layers of rights. William the Conqueror might have claimed that all of England was held of him, but not that it was his. While students of feudalism are happy with such layers of rights embodied in enfeoffment, it is too often overlooked that there were numerous layers underneath, each becoming more concrete, more immediate, more nearly what we would call ownership.

Alan MacFarlane (1978) has long argued that English medieval peasants regularly bought and sold 'their' land, even when it was held of villeinage tenure from their local lord. In short, the land changed hands but the lord's rights to villein labour remained. The same was true of earlier periods. Herlihy (1960) gets somewhat confused when he exclaims that tenants colonising a lord's waste land had amazingly unrestricted rights when they could sell another person's property. Of course they did not sell someone else's property; they sold their land and tenancy together. Buying and selling a tenancy might be restricted by the lord's wishes, as often seems to have been the case in Merovingian Gaul. But even if the seigneurial right was flaunted, or if it was not enforced, or if it did not exist at all, the change of peasant hands did not necessarily mean any loss of the lord's rights to rent, renders, or labour. In short, the right to sell, or even the act of selling, does not prove the freedom of the property from obligations.

This all causes enormous difficulties when scholars attempt to locate small independent autonomous farmsteads. When M. Tits-Dieuaide (1985, 41) and D. Hägerman (1985, 57) both find the model formula for the sale of *manso nostro in pago Avernico, in vico illo, in villa illa* in the *Auvergne Formulary* of the sixth century, they both conclude that they have found an independent peasant property. But not just an independent *mansus*, but one which was located within a villa, one which was on an estate owned, presumably, by a great lord, yet free of the 'lordship'. But there is more than one objection to this interpretation. There is the possibility that the noble husband and wife might have been the owners of the villa itself and were only donating one *mansus* of it. As Doehaerd (1971, 164) observes:

The formularies furnish without doubt a large number of contracts of sale from one vineyard, one house, one piece of land, a few farms! But is it a case of small landowners? One cannot tell for these models do not place the position of the properties within the total patrimony of the seller.

And yet there is another possible interpretation, so often missed: the gift or sale of the *mansus* might have transferred with it whatever obligations and dues were customarily incumbent upon the couple. For all we know the model might have expected the villa lord to have been the first witness. The formula is quite in keeping with seigneurial claims that no *colonica* should be sold by a dependent peasant without permission. However, it is more commonly overlooked that peasants might exchange their property despite seigneurial claims that they should not do so without permission. Thus Charles the Bald's edict of 864:

... in certain places, tenants of royal and ecclesiastical manors sell their inheritances, that is the manses they hold, not only to their peers but also to clerks or village priests and other men. They retain only their homes and thereby *villae* are destroyed, for dues can no longer be levied and it is no longer possible even to know which lands are dependent on each manse.

To this we might add that in Merovingian times we have plenty of evidence that peasants flouted another common seigneurial claim to control, that of marriage. Bearing this in mind we may be able to better understand the problems encountered by scholars trying to compare Merovingian and Carolingian villas.

Merovingian villas are known primarily from charters and wills, Carolingian villas are best known from polyptychs, and each contain different kinds of information. This is no revelation; it has long been recognised. But each deals with different layers of exploitative control over the countryside. This I believe has not been fully appreciated. It is not accidental that the Merovingian charters and wills often named slaves, for they were property. *Coloni*, on the other hand, were not property. Of course lords could sell their villas and transfer the rights of

rent or services from their *coloni* to the new lord. One can still transfer the 'ownership' of people's debts today when selling businesses. This does not amount to 'selling the tenants', it does not make the tenants slaves (although surprisingly this mistake is made on rare occasions by some scholars). When Merovingians gifted, sold, or bequeathed villas they might stick to what was most concrete and most fully owned in their documents, taking it for granted that the dependent obligations went along with them. It was not often that such a full list of what was gifted was made as that of Dagobert's gift of Etrépagney (Eure) to St-Denis in 629:

cum omni integritate et soliditate, hoc est domibus, edificiis, presidiis, mancipiis, colonis, inquilinis, accolabus, libertis, servis tam ibidem oriundis quam et aliundis translatis, rusticis et urbanis, saltis atque subjunctis, terris cultis et incultis, vineis, silvis, pratis, pascuis, aquis aquarumve decursibus, pecoribus, pecuniis, mobile et immobile, omneque genus pecudum et universum merita, adpendiciis, adjacentiis tam intra terminos quam et extra terminos.

Nowhere is there a fuller list of dependent peasantry in any Merovingian charter, formula, or will.

If, however, no peasants but slaves appear in a text, does it mean that there were no serfs? Tits-Dieuaide (1985, 32) quotes the bequest of villa Tresson by Bishop Domnolus in 572: *cum agris, pratis, pascuis, silvis, aquis aquarumve decursibus, cum mancipiis huius nominibus*, followed by eight names. She takes this to mean there were no *coloni*. Given that she accepts that Tresson estate comprised four or five thousand hectares, we might well wonder if the eight slaves were not hard pushed to tend it all! Or was it all waste? The simple fact that many Merovingian charters mention *terra culta* but only a few slaves who were explicitly engaged solely in tending vineyards, means that the land had to be tilled by others. Here I suggest the other were *coloni* or *rustici*. Tits-Dieuaide's attempt to analyse Merovingian villas through the labour exploited is commendable, but she has surely taken the charters too literally.

Scholars have repeatedly looked for '*indominicata*' in Merovingian documents to prove the existence of a demesne worked by the labour due from villeins. The conclusion from their vain search, that there were no labour services, is to my mind quite wrong. Merovingian and Carolingian vocabulary differed. *Curtis* was not a word Merovingian writers preferred over *villa*; *casa* or *domus* was more to their taste than *mansus*. If Merovingian lords did not talk of demesne fields, they nevertheless had plenty of *terrae cultae* that was cultivated by someone.

Recognising this, Tits-Dieuaide assumes small amounts of directly farmed land, worked by slaves. Rather more common, she assumes, was tenanted land

which owed produce renders but no services. This is no more than the received wisdom of generations of scholars who polarise peasantry into labour-owing *servi* and produce-owing *coloni* who were only to melt together in the Carolingian period.⁵ The theory seems a little too contrived, too neat for the minimal amount of evidence available about Merovingian dependent peasant obligations. Such a sharp distinction also seems unlikely given the propensity for early medieval authors to lump *servi* and *coloni* together as rustics. Would this have happened if such a gulf truly separated them?

The concept of an 'agricultural system' has further tangled the web scholars weave. The origin of the 'classic Carolingian demesne system' is a classic question asked by the few scholars interested in Merovingian agriculture. But there was no system as such. Scholars have in fact put themselves into the landlord's shoes, they have donned the mantle of the estate steward of a great monastery, or have seen villas with the eyes of Charlemagne's *missi* sent to record all the royal estates with a parchment copy of the *Brevium Exempla* tucked into their belts. Yes, St-Germain-des-Près had a system. It had accounts. It had land worked by slaves-cum-serfs, land rented to other lords, tenants rendering produce of varying kinds and varying quantities. But the whole landscape was not exploited according to any single system. As powerful as Frankish kings or great abbots were, they could not dictate how all the fields were worked or what form of dependency on them the peasants would have to bear. Seen through the eyes of the peasants the land was worked as small farms. This was the closest thing there was to an agricultural system. The peasants' system was to produce enough to meet the requirements of the domestic unit and pay off all the obligations owed to rapacious lords.

Moreover, there was not even such a creature, the independent peasant, for the Church demanded tithes of all and the king demanded taxes irregularly. These were heavy impositions. By comparison the duty to ride messages for the local abbey were light. Many of the people who appear on the parchment of polyptychs might have considered themselves as independent as was possible for a farmer. A few chickens or eggs at Christmas, a few days work during harvest, this may have fitted into the abbot's system of feeding his monks, but it was probably only a nuisance to the *ingenui* who had inclement weather and pests to deal with, whose system was nothing less than the appropriate balance of crops grown and livestock raised.

5. Rivers' argument, discussed earlier, was the result of an attempt to show how the fusion had already taken place in the Carolingian period, that *servi* were obliged to render produce and *coloni* were obliged to render labour.

The 'system' so many historians talk about is nothing other than the systematic exploitation by great landlords of peasants of varying levels of dependency. Lords were probably uninterested in devising a systematic form of agricultural exploitation. They were only interested in a system of increasing their wealth. Just as well, because far more agricultural production occurred on small farmsteads by their occupiers than on demesne home-fields, and there, on the *mansus*, lords could scarcely affect any agrarian changes.

Manses

Thus we come to the smallest settlement unit, the farmstead. Here one might have thought there would be no complication, perhaps a degree of unanimity, even a modicum of common sense. Not so. While everyone agrees that *mansus*, related to *mansio*, meant 'house' in the physical sense, the fact that 'house' might stand for more has caused no end of trouble and a flood of ink (Dumas 1926; Dubled 1949; Ganshof 1955; Herlihy 1960; Schlesinger 1979). Rather outlandish is the theory of Walter Schlesinger (1979). Because Merovingian charters list *mansum cum pratis, terris cultis*, etc., Schlesinger takes this poor little word 'with' as proof that the Merovingian 'house' did not include fields and gardens. Unlike the Carolingian 'house' it did not mean 'homestead'. One might as well claim that our own expression 'hearth and home' means that we conceive the fireplace to not form part of the house. Perhaps a foreign scholar might postulate that the English hearth is outside the house. This is almost what Schlesinger does, for he further suggests that Frankish farms were *Wohnstallhäuser*, byre-farms, because separate byres are not enumerated in the stylistic legal wording of charters. But archaeology shows this is not so. These byre-houses were restricted to the North German Plain in the early and central Middle Ages (Chapelot and Fossier 1985).

'House' for 'homestead' is a metonymy: *pars pro toto*. We see the same thing with *domus* which Gregory of Tours used frequently for villa. We see the Carolingians adopting *curtis*, courtyard, for villa in the sense of the whole agricultural estate. *Palatium* becomes not just the whole palace complex, it becomes the people of the palace. We call them the 'court' just as the Germans call it the *Hof*: a name abstracted from the physical setting, metonymy.

Tits-Dieuaide (1985) argues forcefully that the list of fields, vineyards, slaves, and houses is not random. Vineyards figure in documented places where grapes grow today and are as absent from the Merovingian estates of the ill-drained northern flat fields of France as they are today. On the other hand, where are the animals in the texts? The livestock must have been gifted with the villas, but are absent from the written record, which is, after all, no more than a rhythmic ritual

litany of the parts comprising the whole. Dagobert wished to say nothing less than I give to St-Denis my *entire* estate of Étrapagny. He simply did it rather more sonorously.

All the problems generated by *mansus* derives from the fact that in Carolingian times it was used as a *de facto* administrative measure. Monasteries with 3,000 to 8,000 *mansi* were 'major', 1,000 to 2,000 were 'mediocre', 200 to 300 were 'minor'. Because *mansus* also first became popular as a term in the Carolingian period, replacing *domus*, *casa*, and *mansio*, some scholars have been led to the amazing conclusion that somehow Charlemagne instigated a land reform and a new unit of measurement (e.g. Schlesinger 1979), when he could not even create a unified *lex* out of the pathetic *Lex Salica*, or that widespread oppression had forced formerly free farmers' farmsteads into an exploitative villa system. If this were so, if *mansi* had been independent farms in the Merovingian period, and if Merovingian landlords had but few slaves and no labour-rendering *coloni*, it would appear that Merovingian lords had almost no dependent peasantry to exploit! Or was Engels perhaps right after all, were the Franks egalitarian peasant farmers, innocent of treating land as property?

Such pseudo-historical semantic commentaries should never have evolved given that the *mansi servile* of Carolingian polyptychs were quite indubitably the descendent tenures of the *servi casati* of Merovingian texts. Not only do they correspond in semantic derivation to 'servile house' and form dependencies on aristocratic villa estates, but the *servi casati* had become part of the 'immobile' element of Merovingian villas according to our sources. They were not just 'slaves in houses', for the sources make it clear regularly that these slaves could not be sold or gifted away from the land. That is to say *servi casati* were indeed dependent tenures.

Whether archaeology will ever contribute to a better understanding of these farms of dependent peasants remains to be seen, and probably not for many years yet. But it must be remembered that it was the exploitation of these humble homesteads that made the residence of the *potentiores* what they were. And what they were is what we turn to next.

MEROVINGIA

Chapter Three

Post-Roman Villas: the Fifth Century

Unfortunately I must leave that favoured land [Italy];
Gaul, where I was born, summons me away.
Long wars have ruined the fields of my native land;
pity takes me from the land that I love.
It is nothing to neglect men who are at ease,
but suffering compels our loyalty.
An ancestral home needs our presence and our tears;
labour which grief has urged is often best.
It is sinful to neglect ruin already
compounded by neglect: now is the time,
after the fires have cooled, to rebuild, even if
we are rebuilding only shepherds' huts.
(Rutilius Cl. Namatianus *De reditu suo* 1.21-32, ca. AD 416)

The transition from Roman to Merovingian villa is poorly understood and the intractable evidence of the fifth century would seem to hold the key to the solution. Intractable, for the archaeological evidence largely ends with the fourth century and documentary evidence begins with the sixth. In between we are left mainly with tantalising fragments and elaborate theories. What helps to make the subject slightly less unmanageable is the important synthesis by John Percival (1976) and much of the illustrative material given below is perhaps best seen as complementary to Percival's work.¹

The belief has long existed that Roman villas were finally and irrevocably abandoned or destroyed in the late fourth or very early fifth century. The year 400 has been taken roughly to mark the final demise of what we understand as the Roman villa and villa system. The evidence for the belief is quite overwhelming: many dozens of *excavated* sites, none revealing conclusive evidence of the buildings of a Roman villa continuing in occupation well into the

1. A short summary article on fifth-century Gallic villas by Percival is soon to be published in a volume edited by J. Drinkwater and H. Elton on fifth-century Gaul. I have not seen the typescript but have heard the contents delivered as a paper at Sheffield 1989.

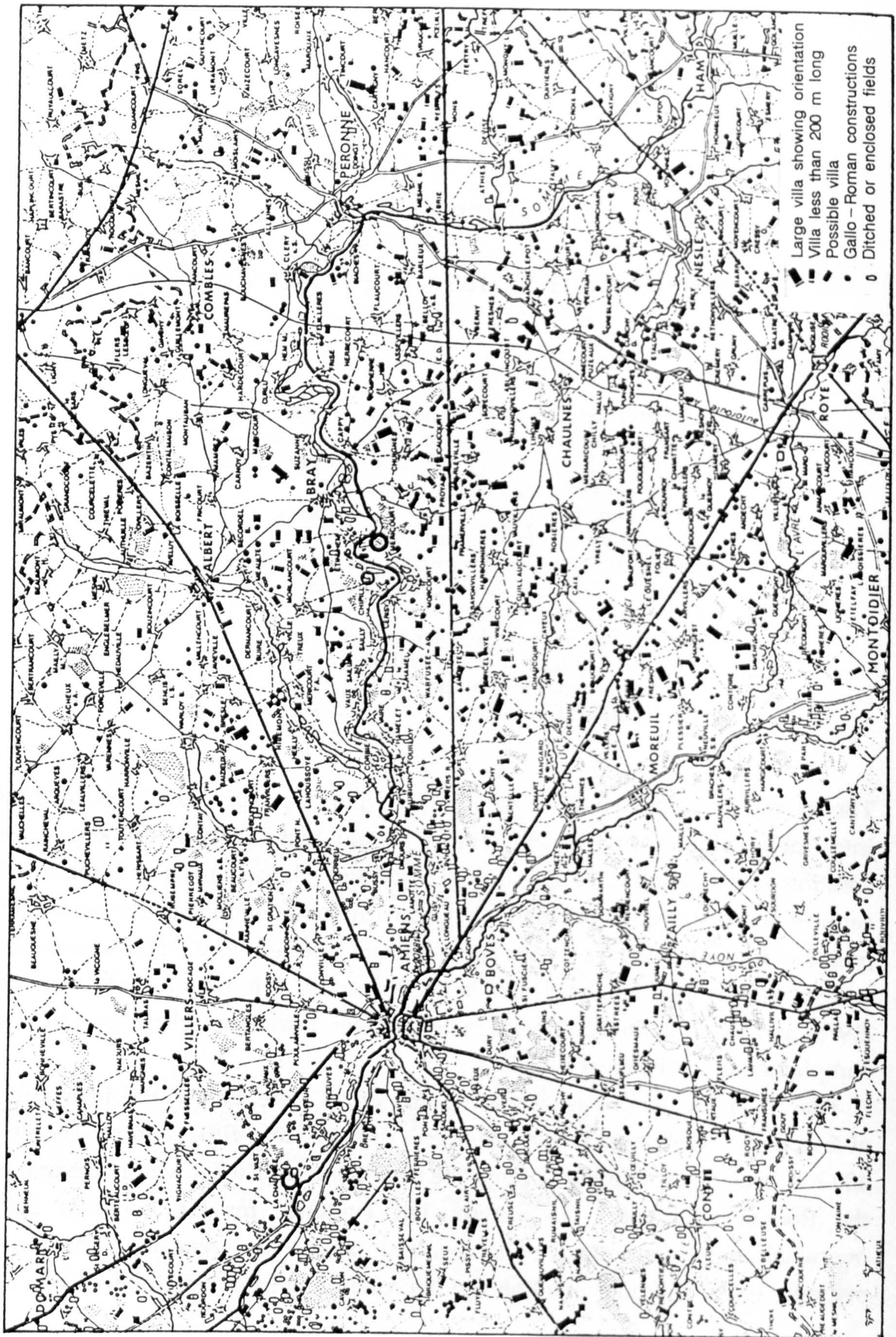


Fig. 3.1 Villas and other Roman settlements in the Somme valley detected by aerial photography 1:100,000 (after Agache).

fifth century or beyond. All the excavated sites of western Europe graced with the title of Roman villa seem to fall into ruin during the third or fourth century. The phenomenon is quite simply too well known to need the quotation of either examples or leading authorities. There is further the striking evidence of dense villa settlement in Picardie as revealed by the exciting work of Roger Agache (fig. 3.1). It appears to underlie the modern French landscape without bearing any relationship to it. The very clarity of the picture seems to contradict any claim to continuity. The *Carmen de Providentia Dei* details the loss of fine things a villa owner might have suffered in the calamities of the fifth century. The villa itself is burnt out, the servants are shabby, and the fields are overgrown. It is only one of a series of fifth-century poems filled with metaphors likening the destruction of the material world – ‘only recently have solidly built villas been destroyed’ according to the *Epigramma Paulini* – to spiritual decay in the country. They sum up neatly what is generally thought to be the fate of fourth-century villas.

Although archaeological recognition of fifth-century or later occupation on villa sites which are known to have been occupied in the third and fourth centuries is extremely rare, stray finds of early mediaeval date are not (Vieillard-Troiekouroff, pers. comm.). Percival (1976) notes some of the best-known examples: Nennig (Rheinland-Pfalz), Berthelming (Moselles), Anthée (Namur), Montmaurin (Hte-Garonne), Beaucaire (Gers), Colleville (Seine-Maritime), and Pujo (Htes-Pyrénées). Although the evidence is always equivocal and meagre, it is striking how widespread the phenomenon is. Even across the Rhine in the *agri decumates*, lost to the empire in 260/1, there is settlement debris found on top of Roman villas (see chapter five). At Ladenburg (Rhein-Neckar-Kreis, Baden-Württemberg) Alamannic finds come from the *villa rustica* and from three other areas all within a 200 metre radius (fig. 3.2), one of the areas also yielding a *Grubenhaus*.

A short excursion south of the geographic borders set by this thesis offers interesting parallels and some rough statistics. The villas of the Iberian peninsula have been recently catalogued by Jean-Gerard Gorges (1979, 56), who notes:

Of 140 villas well attested for the fourth century, a little more than a third only (about 50) appear to have continued into the fifth century. This does not signify the total disappearance of villas, but indeed a spectacular diminution of this type of (agricultural) exploitation and its evolution into a new type of structure. We have, for some ten sites, archaeological proof of *dominicale* occupation or of a seigneurial and artistic development in the fifth and sixth centuries: Pises, Banos de Veleardos, Aguila fuente, Rioseco de Seria, Alcala de Henares, Albasa, Aytona, Albalate de Cinca, Estada, Santisteban del Puerto These are the great estates of late Antiquity.

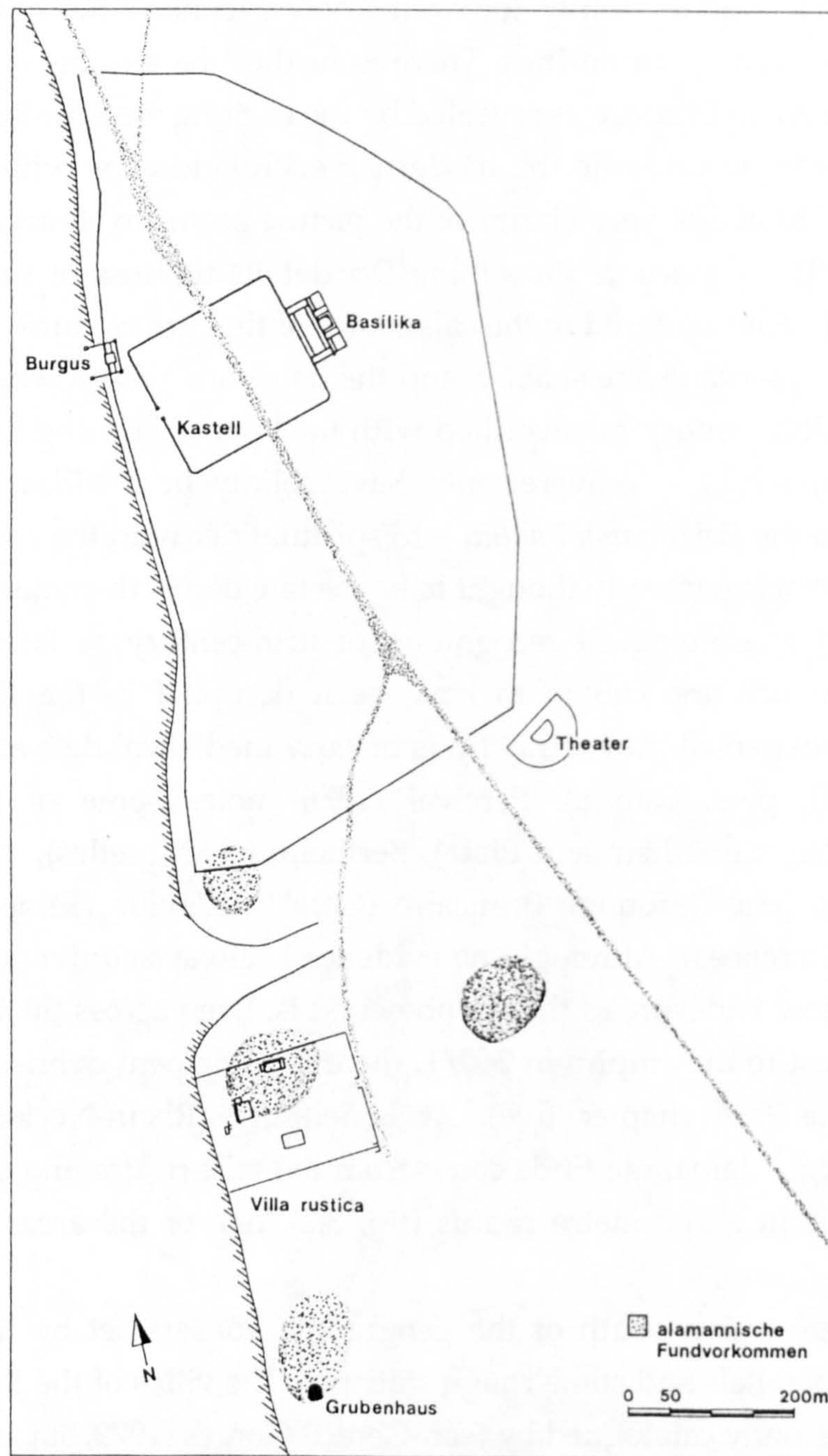


Fig. 3.2 Ladenburg (Rhein-Neckar-Kreis, Baden-Württemberg). Alamannic find-spots and Grubenhaus by a Roman villa rustica and settlement by a late Roman fort (after Schallmayer 1986).

Recognition of such occupation relies most commonly on late ceramics. Thus for the province of Seville, Gorges notes that 85 sites are attested in the fourth century, but only 12 yield '*sigillée claire estampée rouge*' of the first half of the fifth century and only one reveals later '*ceramique visigotique*'. The fact that fifth- and sixth-century pottery is found on nothing like the scale of fourth-century pottery is best interpreted as reflecting the marked decline of pottery production. It would be wrong, therefore, to interpret the decrease of sites in Seville yielding pottery from 85 to 12 to 1 as representing the actual proportions of villa occupation and desertion. This claim is perhaps strengthened by the fact that almost without exception those ten villas signalled by Gorges as yielding evidence of fifth- and sixth-century occupation, fell into the restricted category of 'villa of recognised importance whose site has been partially or totally excavated'. The two lesser categories ('sites yielding numerous finds and thought very likely to be villas' and 'sites yielding fewer finds the interpretation of which is equivocal'), which make up much the largest percentage of the catalogue, provide little evidence of later occupation. The conclusion to be drawn is that only under intensive investigation does the evidence of post-Roman material come to light. Pottery spectra are biased against such archaeological recognition. Indeed, one Spanish site, Vilauba, which has been investigated since Gorges's publication, which gives startling evidence of continuity and will be considered later in this chapter, only did so after careful excavation.

The difficulties in spotting fifth-century occupation caused by the dearth of readily recognisable and datable artefacts is highlighted in Aquitaine. There one finds a debate over the dating of late villa mosaics of a type similar to those found at Séviac. The black and white, geometric-patterned mosaics are commonly said to date to the fifth century. Such mosaics date a few of the fifth-century villas of Spain too (Gorges 1979). If this is accepted for Aquitaine it puts a substantial number of villas otherwise datable to the late fourth century into this dark Visigothic period (fig. 3.3).

Against the picture of apparent discontinuity of villas from the late Roman into the early mediaeval period, there has long been known the testimony of Sidonius Apollinaris and Venantius Fortunatus who describe the villas of Sidonius himself, of Pontius Leontius and Consentius in the later fifth century, and of Bishops Nicetius and a later Leontius, bishop of Bordeaux, in the sixth century. In the case of Sidonius at least, the descriptions given are unmistakably those of what we would call a Roman villa. The question is, where are they?

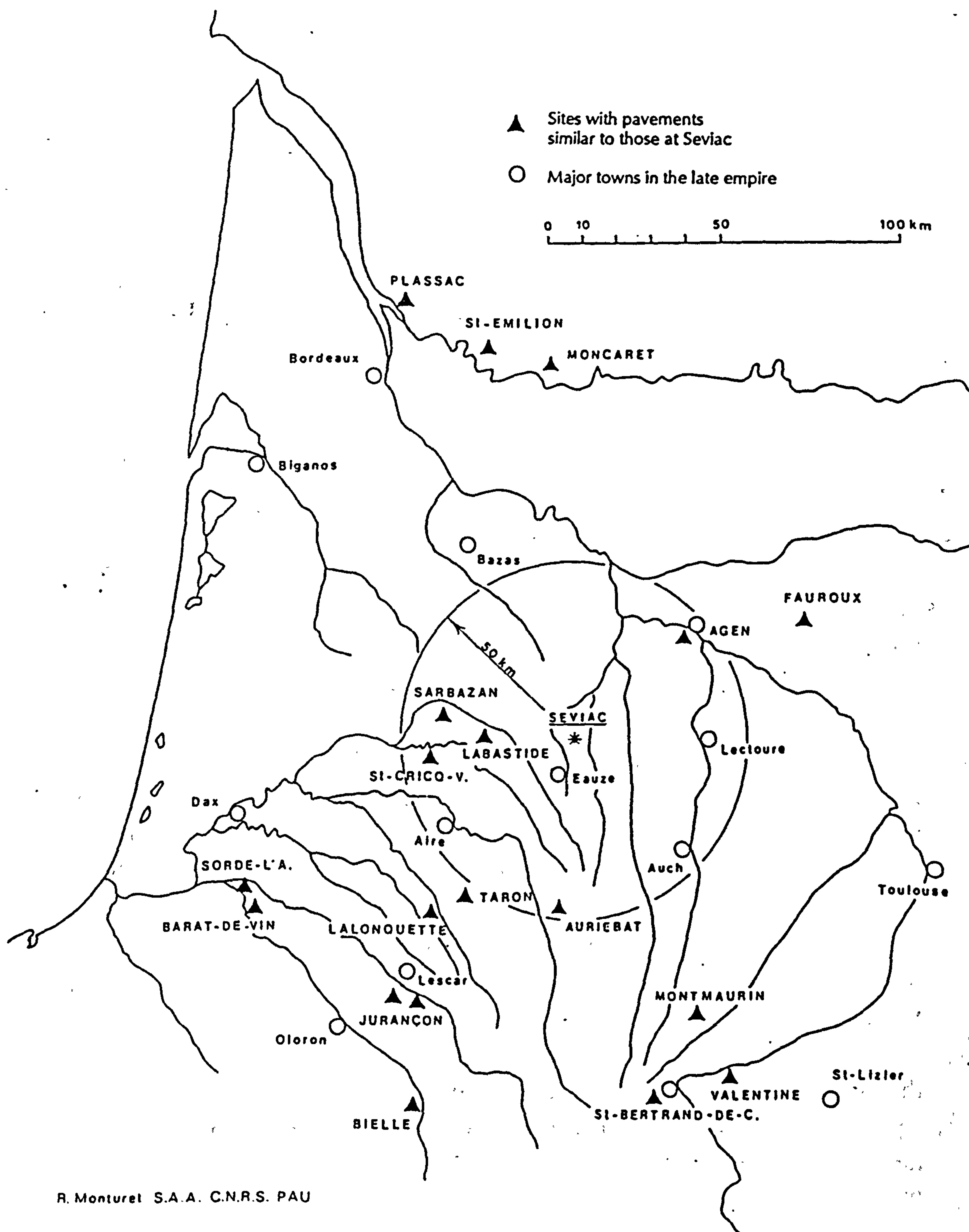


Fig. 3.3 Late Roman villas with mosaics of the type found at Séviac in Aquitania (after Monturet).

Villas under Villages

There also appears to be continuity of Roman estate names (or at least a Roman naming tradition) into the early mediaeval period. *Villae* abound in sixth- and seventh-century documents, in some seventy per cent of the cases with a Gallo-Roman name (Bergengruen 1958, 60). Numerous French villages survive to this day with various endings: -ay, -ac, -é, -ey, and so forth, depending on the region, derived originally from the Latin ending *-acum*. It is well known that Roman villas were often given names derived from that of their owner with an *-acum* ending added. Thus Sidonius's famous villa Avitacum came to him from his wife, the daughter of the short-lived emperor Avitus. The modern Loupiac near Bordeaux, yielding a large Roman villa (as well as world-famous dessert wines), would thus plausibly owe its name to Lupus. The custom would seem to have been borrowed by the Franks. Indeed almost the only Frankish place-names to be found in France are derived from personal names and these then combine with Latin elements: Bertoncourt or Landreville from Berto and *curtis* or Landeric and *villa*. De Coulanges accepted such *-acum* names as yielding unshakable proof of Roman to Merovingian continuity. More recently Michel Roblin (1951; 1978) frequently relied on *-acum* names alone as proof of the existence of a Roman villa to recreate the Roman and Merovingian landscape of the *départements* of Seine, Seine-et-Oise, and Oise, although he was occasionally at pains to reconcile what seemed to him contradictory evidence. While other scholars have been less emphatic in their belief that *-acum* names invariably owe their origins to a Roman villa and represent continuity, few have taken an opposing stand. Alexander Bergengruen (1958) was one. He firmly disbelieved that sixth- and seventh-century villas ending in *-acum* were successors to earlier Roman estates. However, his theories rested on a view of the fifth century as an unmitigated disaster resulting in total destruction of villas, widescale abandonment of fields, and a drastic reduction in the population. The sixth century by contrast was the new beginning of economic and agricultural growth, in his eyes. In short the fifth century, devoid of the documentary evidence Bergengruen was using, was supposedly a razed plain on which a new social order could be built. Clearly any theories which rely on the assumption that the fifth century swept away an old civilisation and provided a clean slate for a new one will be doomed to failure. However, Bergengruen is not to be completely dismissed, for his approach was to probe the evidence of the first two or three post-Roman centuries very closely and rely less on *a priori* theories of continuity or discontinuity, and a few of the conclusions with which this chapter will end echo his sentiments.

Do the numerous French villages that bear names that once ended in the Latin

-acum and the large percentage of Merovingian estates that bear Gallo-Roman names really constitute evidence of the continued survival of late Roman villas into the early Middle Ages? With little doubt it is necessary to test the hypothesis that *-acum* names really do relate to an original Roman villa. One clear proof would be the archaeological discovery of Roman villas in villages with *-acum* names. Percival (1976, 173) shows that north of Melun where some eleven villas have been found, at least four have place-names which originally ended in *-acum*. Otherwise he notes that there are 'a number of other places' in France, which include Frontenac, Loupiac and Plassac all in the Gironde *département*. While this is suggestive, perhaps the closest thing to a test by excavation is the situation in the *limes* region of Switzerland. Of 86 Roman villas underlying modern settlements 37 per cent are found under villages with names of Romance origin. This is of interest because 85 per cent of the village names in the region are Germanic, in stark contrast with northern France. More importantly, of the 30 villages with *-acum* names, Roman villas have been found under 16, or just over half. If, however, modern *-acum* places really do owe their origins to a Roman villa the chances of detecting it are slim, for continuous occupation for a further one and a half millennia has very likely removed all trace of that origin. Perhaps the other 14 Swiss *-acum* named villages overlie Roman villas but the evidence has been destroyed.

Nevertheless there is one way of testing the possibility that the villages with *-acum* names that yield no archaeological evidence of a Roman villa do indeed mark the site of such villas. Percival (1976, 180-2) suggests that *-acum* villages could represent 'lost' villas on distribution maps. Just south of Paris he notes *-acum* villages outnumbering known villas by some twenty to one. If they all represented the descendants of villas the map becomes very crowded with villas appearing at intervals between 1,500 and 2,000 metres apart. This kind of density is just what we might expect as we grow accustomed to the startling distribution of villas south-east of Amiens recognised by Agache during aerial reconnaissance. The *-acum* names also produce a very homogeneous dispersal, unlike the inexplicably concentrated distribution of archaeologically known villas. Percival similarly reveals the sense of accepting *-acum* names as former villa sites in Hainault, Belgium, without which the known distribution would partially avoid the most favourable arable land. Finally, this 'negative' indication can be taken a step further. Something like an inverse relationship of the number of excavated villas to *-acum* place-names exists. Thus when continuity, as expressed in the place-names, is more marked the number of 'failed' sites, as expressed in archaeologically visible remains, decreases.

It must be confessed that an uncritical acceptance that *-acum* names genuinely reflect the sitings of Roman villas, and are thus proof of continuity into post-Roman centuries, creates problems. Mentioned above was Percival's discussion of place-names south of Paris, an area which has been intensively studied by Roblin. It has already been noted that Roblin relied heavily on such place-names as being a clear indication of a late Roman villa, and it is not surprising that most of the sites figuring on Percival's map, figure in Roblin's book as the centres of *fundi*. L'Hay-les-Roses, Antony, and Massay among many others are accepted as Roman villas without question, while others such as Noisy, Savigny, and Bretigny are thought likely candidates, although they appear only very late in documents and their supposed previous pre-eminent position in the region is based solely on their *-acum* names. There are, however, others such as Rungis, Orsay, Epinay, and Saquiniacus which Roblin finds appearing not only late in the documents but in unfavourable geographic settings, in the case of Rungis with an incomprehensible name and with little or no circumstantial evidence to suggest that the site was ever prominent in its immediate locality.

In the case of Roblin's work the micro-topographic history of Roman settlement was written from late mediaeval place-names; Roman villas were sited in untypical villa settings on the strength of modern village names ending in *-ay*. This should not be allowed. Percival expressly avoids the contention that *-acum* names may be used as proof at the level of individual sites. He envisages a portion of the villages as having acquired *-acum* names while owing nothing to previous Roman villas. The technique is thus to be seen as yielding meaningful results only when a larger landscape is viewed. The theory, however, relies on the belief of continuity of occupation at the site level, even if it has to speak in quasi-statistical terms.

The transformation of the Latin word *villa* into the French word *ville* has long led to the speculation that a comparable metamorphosis occurred in the physical world. Why such a process has left so little archaeological trace is the subject of several other theories. It was noticed at the start of this chapter that the picture of villa settlement in Picardie, as revealed by Agache's aerial photography, suggests that there was a fundamental break in the settlement pattern at the end of the Roman period. Admittedly at first sight the prospects for the defenders of continuity seem bleak, but Agache offers encouragement to this party. Of importance is the question, just what period does the settlement pattern represent. Of the large villas, Agache (1983, 25) suggests that 'the majority of those visible from the air were destroyed in the second half of the third century.' Not only does he suggest that these large villas represent the situation in the

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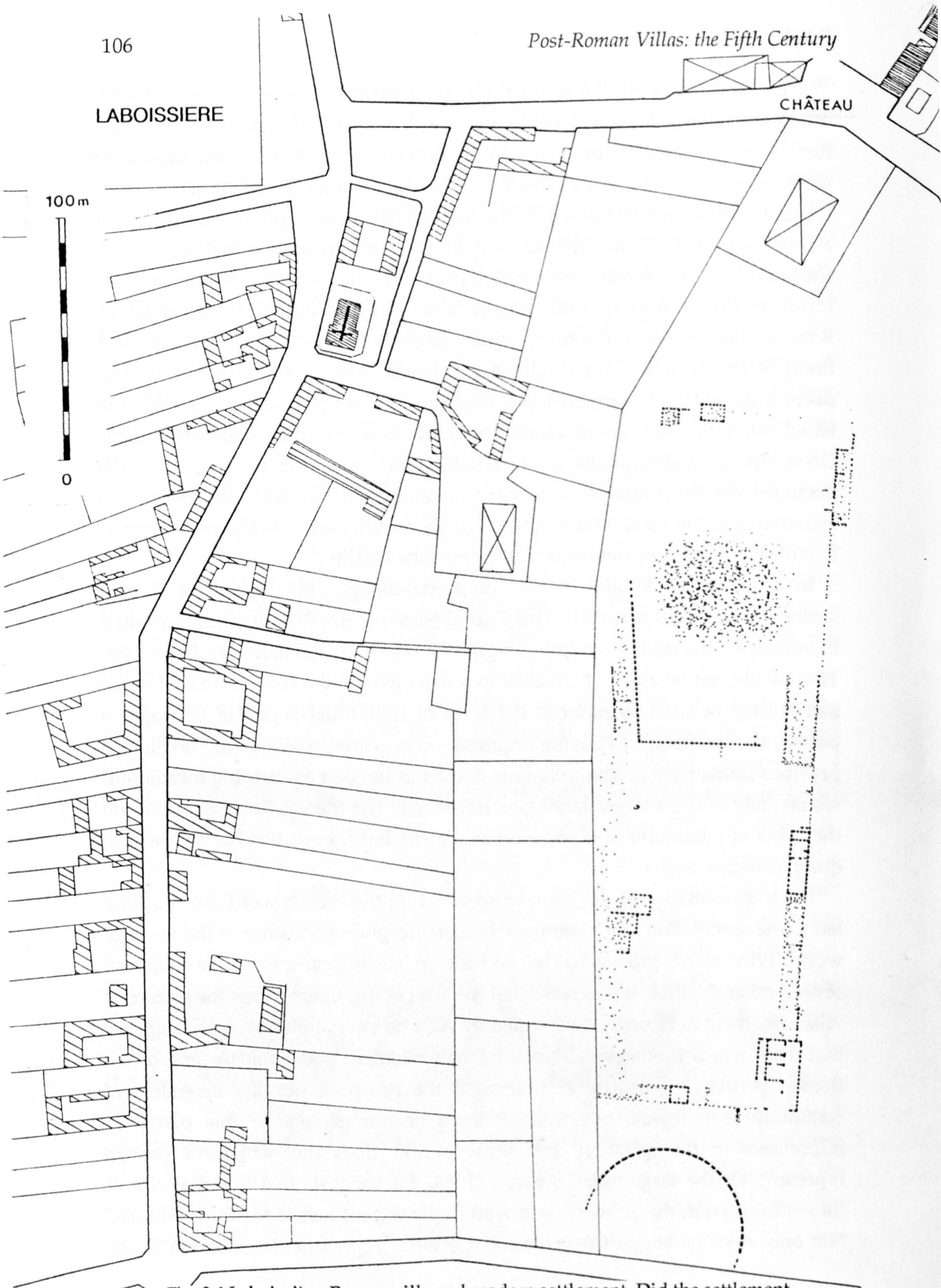


Fig. 3.4 Laboissière Roman villa and modern settlement. Did the settlement originate outside the villa courtyard walls? (after Agache)

latter half of the third century, but further that those which survived or were reconstructed, were indeed the ancestors of modern French villages. Such a suggestion is based on excavation and field-walking programmes designed to date these aerially discovered villas.

Agache notes that under many of the deserted mediaeval villages recorded by his aerial reconnaissance, there is a Roman villa to be seen. Among his examples is Laboissière (Somme) (fig. 3.4), where he assumes the modern village owes its origin to an ancestral settlement which began growing outside the villa walls while the villa was still inhabited. Other odd spatial relationships between Roman villas and modern village plans similarly speak for continuity. In Switzerland the villa in Bernex (GE) would appear to have influenced the direction of major roads. One road leaving Bernex north-westwards towards Aire-la-Ville can be postulated to have once run directly through the courtyard gate of the Roman villa (fig. 3.5). Of particular interest, in view of the importance royal Merovingian villas will play in the next chapter, is the example of the modern village of Kirchheim (Alsace) overlying a Roman villa (Plath 1904). Here the apse, hypocaust, water conduits, and wall paintings from a bath have been found and a Roman road passes the village. A Merovingian sarcophagus is still to be seen in the village and a Merovingian cemetery has been discovered at a neighbouring village, Odratzheim. A late tradition had Dagobert donating Kirchheim to St. Florentius for having cured his daughter; while Florentius rode a donkey around the boundaries of his new estate, Dagobert bathed in Kirchheim. A Roman villa has also been excavated just over a kilometre from the village of Athies (Somme), which was a royal Merovingian villa (see below). Athies village is to be found in the valley. Similarly at Le Mesge (Somme), less than a kilometre away from the large Roman villa, the village is in a valley. Again it appears in the documentary sources as a royal Merovingian villa (see below).

One of the conclusions Agache (1983, 26) draws is 'that the large domaines continued into the early Middle Ages but that . . . more so the permanence of the *grandes propriétés foncières* than the persistence of the large villas themselves, which seem to have shifted location somewhat.' While this is an interpretation much loved of those who try to reconcile the now popular view of continuity with the undeniably rare evidence of villa buildings surviving into the fifth century and beyond, it is also almost impossible to prove. Continuity of estate boundaries is impossible to demonstrate when we cannot honestly claim to know the fixed boundaries of any single Roman or early mediaeval villa. This will become more apparent in the next chapter. For now we must continue our investigation of fifth-century villas by turning to some individual sites.

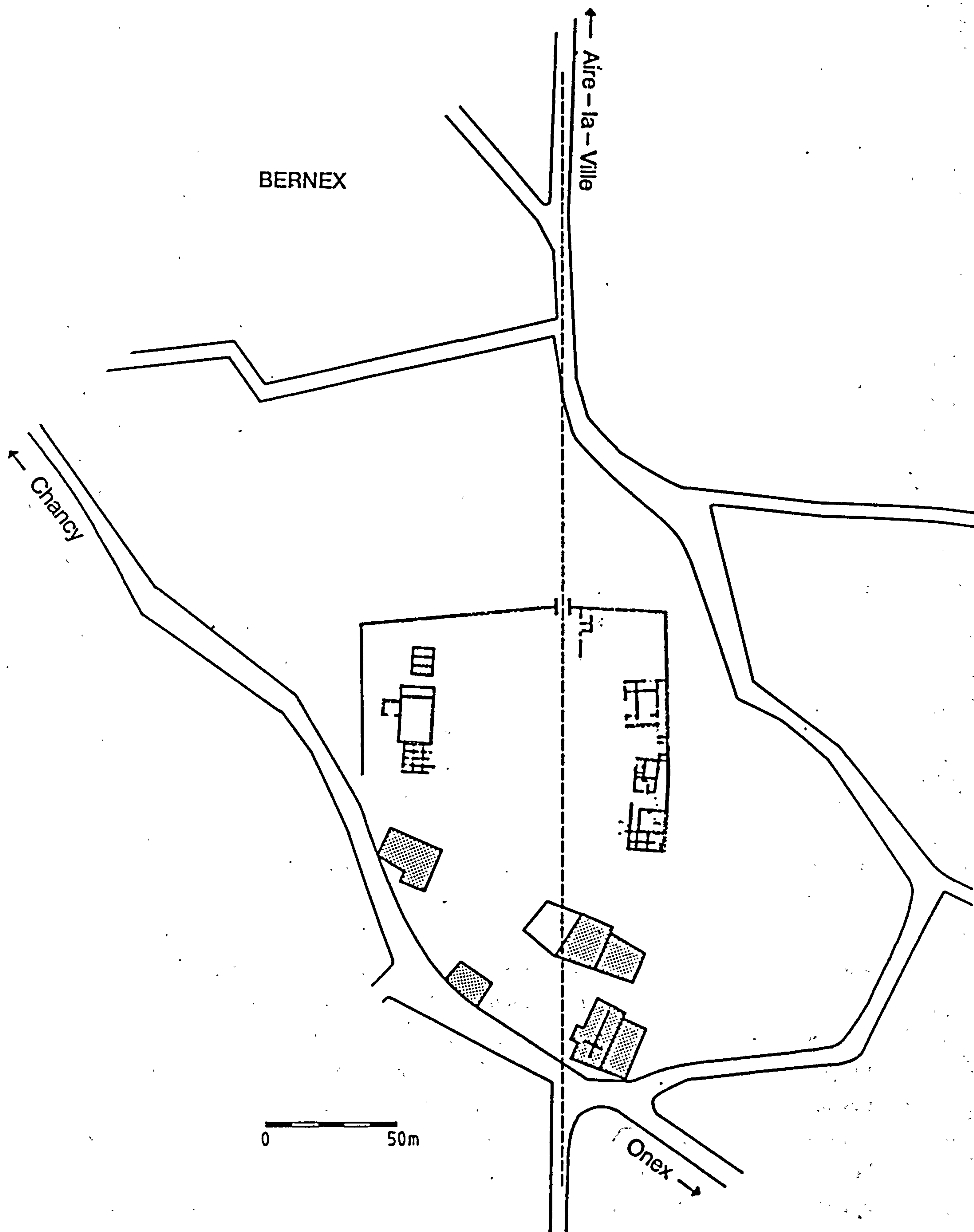


Fig. 3.5 Bernex Roman villa and modern village. Note the relationship between the villa gateway and modern roads (after Martin).

Villas and Churches

Perhaps one of the most striking archaeological phenomena in the realm of post-Roman occupation of Roman villas is the frequency with which churches are met overlying them. Returning, as one is forced so often to do, to Percival, we find a whole chapter (1976, ch. 9) dedicated to the relationship of villas to churches and monasteries. The number of villas which are associated with Dark Age burials in France and Belgium would seem to number in the hundreds and Percival rightly notes the oddity of the claims of their excavators that Berthelming (Moselle) and Pompogne (Lot-et-Garonne) were continuously *occupied* on the evidence of corpses buried within various villa rooms and even along wall foundations. A number of interpretations can be offered. One has been that the villas were continually exploited agriculturally either by surviving inhabitants or newcomers. Although the buildings might be abandoned as they decayed for the want of technical or financial capacity to maintain them, the fields are still seen to be ploughed, the meadows mowed, and the pastures grazed. The stone-strewn ground of the old villa was thus seen as wasted farm land and a very suitable place for burial, being good for little else and offering guaranteed, undisturbed, final rest (from the plough at least), as well as offering that historical touch to give 'roots' to those who claimed ownership. Another explanation is the continuity of religious associations. Thus the villa of St-Aubin-sur-Mer (Calvados), which has been suggested to have had connections with a now submerged series of *menhirs*, was originally a Celtic temple. A villa was built over it, retaining the shrine, and a large cult statue of a seated female and well were added. The religious associations may have survived until the time of the burials in the ruins of the villa. Ruins may well have themselves been endowed with religious qualities by the fifth- or sixth-century local inhabitants. Intriguingly, burials are often found in parts of the villa whose ground plan would be suggestive of a basilica with rounded apse: in short, a building or ruins which would have seemed appropriate as a church. At Montferrand (Aude) the burials along the wall foundations make it clear that there was no upstanding building. Percival quotes an example of the possible motivation from Gregory of Tours (VP 15.1):

He [St. Senoch] found in the territory of Tours old walls and by restoring them from ruins he made worthy dwellings. He also found an oratory in which, it is said, our illustrious St. Martin had prayed. He restored it with much care and having placed an altar inside which had a small compartment suitable for containing relics, he invited the bishop to come and bless it.

Whether true or apocryphal the story reveals that a Merovingian author believed that locating a ruined oratory among other ruined buildings was fairly

straightforward. What is more intriguing is the possibility that Merovingians mistook the dining rooms of Roman villas, with their rounded apsidal endings, for ruined churches.

Percival is able to give the example of Ligugé (Vienne) where the archaeology almost retells Gregory's story verbatim. It differs only in that the site does not remain a simple chapel but developed as one of the oldest Gaulish monasteries. The existence of churches built on the ruins of villas is a well known phenomenon, for in addition to the example of Ligugé, Percival also notes the examples of Martres-Tolosane (Hte-Garonne), Erôme (Drôme), Ste-Colombe (Gironde), Moissac (Tarn-et-Garonne), Noroy-lès-Jussey (Hte-Saône), Izaux (Htes-Pyrénées), Callas (Var), Puységur (Gers), Flayosc (Var), la Roquebrusanne (Var), Prusly-sur-Ource (Côte-d'Or), St-Symphorien (Morbihan), Bouxières-aux-Dames (Meurthe-et-Moselle), Trinquetaille (Bouches-du-Rhône), Montferrand (Aude), Montcaret (Dordogne), St-Ulrich (Moselle), Eschau (Bas-Rhin), Loupiac (Gironde), Kergollet (Côtes-du-Nord), Sorde-l'Abbaye (Landes), and Morken (nr. Köln).

A similar situation is to be found on the Iberian peninsula where Gorges (1979, 105) notes that the churches built on the ruins of villas in the '*époque paléochrétienne*' are legion: the basilica of Torre de Palma, also the churches of Villarubia, Verin, Fraga, Veilla de Cinca. It is not made clear, but it would seem that this list only includes those which reveal evidence of ecclesiastical construction in the earliest Christian era, thus sites on which churches now exist but without archaeological proof of their earliest construction have been omitted. Their inclusion, however, could be wished for, to allow comparison with Percival's impressive list.

The discovery of Roman villas under churches, and particularly those yielding evidence of great antiquity, is even more marked in Switzerland (Ita 1961; Vogt 1968). At Bettenach, St. Nicholas's church lies on almost the same orientation as the Roman villa beneath it. The earliest datable stone phase of the church is of the ninth century, but even earlier mediaeval graves have been found. At Commugny (VD) the continuity is inescapable (fig. 3.6). Under the modern village and church are the remains of a Roman villa, on top of which a sixth-century church was built. The church appears so integral to the villa that it is hard to avoid the conclusion that the villa was still standing.

Percival offers two possible explanations for the relationship, in addition to that of coincidence. The first is that the late Roman villa may have continued in existence beyond the fourth century in possession of a church, the villa in the course of many centuries would have undergone many changes before ultimately forming the ancestor of the modern village. The other is that the late Roman villa

COMMUGNY

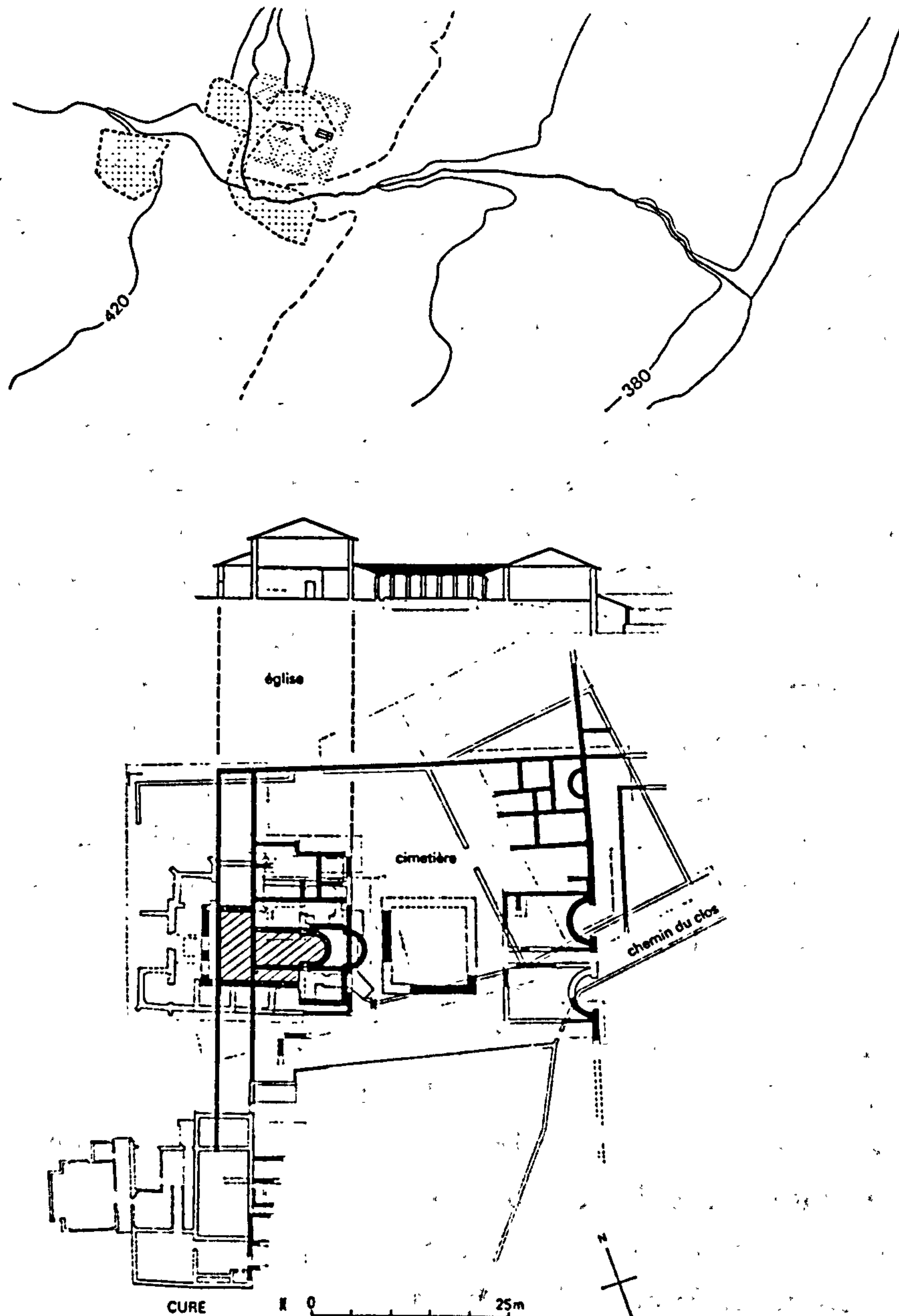


Fig. 3.6 Commugny. Above: the topographic position of the Roman villa (fine stippling) and the present-day village (coarse stippling), including the parish church (St. Christopher). Below: the sixth-century church (hatched) set within a large hall of the Roman villa (black) with later and modern ecclesiastical buildings (white) (after Martin).

actually *became* a church, that is to say, a monastery. The monastery itself then formed the nucleus of attraction and was ultimately the ancestor of the present day settlement. Admittedly our ability to distinguish between the two is very limited but perhaps not really necessary. Monastic sites, early mediaeval palace complexes, or private residential villas furnished with a church will surely all reveal functional differences, but all are assumed to be supported by and to be the administrative centres of, agricultural estates. There may be assumed, therefore, a fundamental, qualitative similarity which will allow them to be treated together.

Before turning to a few archaeological examples, it would be best to outline a brief history of the relationship between private early mediaeval villas and churches, for which the most lucid account remains the work of Imbart de la Tour (1899). For Imbart, 314 AD and a canon of the council of Arles mark the first mention of rural churches in Gaul. Before the end of the century we have evidence of churches and chapels being built on private estates. Thus Paulinus of Nola reveals to us the chapel which Sulpicius Severus had built at his residence of Elusio and the two churches he built on his *ager Primuliacus* (*Epist.* 32). The *Theodosian Code* (16.2.33) refers to *oratoria* built on the private property of individuals and the council of Orange (441) found it necessary to threaten to cut laymen off from communion with the faithful if they invited a bishop from outside the diocese in which their property lay, to come to dedicate a church they had had built. Sidonius Apollinaris refers to the *oratoria* found in the *villae* of senators and mentions his travels during which he celebrated mass at such chapels. It was in the sixth century that such chapels can be seen to have obtained their own priests, and thus were not served by clergy from either the episcopal church or a parochial church usually found in a *vicus*. The council of Albon (517)¹ declared that the founders of such churches, the *possessores* should ensure the maintenance of the priest. By the fourth council of Orleans (541) a canon (c. 33) made it necessary that anyone who had or wanted to have a *diocesis* (it was not until the seventh and eighth century that the terms diocesis and parish reversed their meaning to become that of today's use) on his property would have to assign to it sufficient land to allow the attached priest to perform his office. The transformation of the private church from chapel to parish was complete. The same council stressed the bishop's right to approve the appointment of the new priest in such churches, implying the recognition of the founder and subsequent descendants' right of presentation.

The existence of private chapels or parish churches built on private estates is easy enough to prove. It is much more difficult to estimate how common the

¹ St-Romaine d'Albon (de la Tour 1900, 183).

practice was. De la Tour (1899, 36-7) saw four ways in which rural churches were founded in the fifth century:

1. In a *vicus* or *castrum* by the bishop and inhabitants.
2. On *ager ecclesiae* by the bishop.
3. On a demesne, *vicus* or *villa*, by a large landowner, cleric or layman.
4. In *loca deserta* by a recluse or monks.

It was, however, above all (1899, 56) 'in the *vicus* that we must search for the most ancient centre, the most frequent centre of the parish.' Gregory of Tours mentions more than forty such churches and even uses *vicus* occasionally as a synonym for parish. Nothing reveals better the extension of churches through the countryside than Gregory's concluding chapter (HF 10.31) on the list of the bishops of Tours. Litorius the second bishop converted the first church from the house of a senator. Saint Martin, the third, built churches in a number of *vici*: Langeais, Sonnay, Amboise, Ciran-la-Latte, Tournon, and Candes. The fourth in the *vici* Clion, Brèches, Ruan, Brizay, and Chinon. The fifth in the *vici* Braye, Yzeures, Loches, and Dolus. The sixth in the *vici* Esures, Mougou, Barrou, Balsemes, and Vernou. By now we have reached the last decade of the fifth century and the succeeding bishops are more often recorded founding new *vici* than building churches in old ones. Thus we find the *vici* Manthelan, Neuilly, Luzillé, and the second *vicus* of Neuilly all constructed in the next half century. At the same time we hear of successive bishops bequeathing all their land to the nearest church, but seldom is it a case of leaving their land to churches built on their own land, but rather to these churches of the *vici*.

De la Tour (1899, 58) writes, 'we do not believe, however, that the parishes established in *villae* were very numerous.' That judgement was inclusive of the sixth and seventh centuries. In the latter half of this chapter we will see some of the evidence which indicates how seldom villas were furnished with churches during the Merovingian period. The period of extension out of the *vici* and into the *villae* was seen to be the Carolingian period. The dismemberment of old parishes was then such a commonplace practice that Hincmar wrote a treatise on how it should be done properly and argued that it should not be done unless absolutely necessary. Charlemagne's *Capitulary of Frankfurt* (c. 54) of 794 states:

Concerning churches which are built by free men; it is allowed to bestow them as gifts, or to sell them, provided that no church is destroyed and the daily offices are observed.

Another capitulary forbids the division of these *Eigenkirchen* between lay owners.

This brief history holds a number of implications for the archaeological and historical evidence of villa-church relationships. It will be accepted here that few

villas were furnished with churches in the late fourth and fifth centuries, that the existence of a church on a Roman villa site does not imply their original contemporary existence and that such a relationship implies the foundation by a great landowner, lay or ecclesiastic. What the implications are, will be seen most clearly in the following discussion of a few archaeological examples.

Echternach (Luxemburg). The Roman villa of Echternach lies by the bank of the Sauer, surrounded on all sides by hills, seventeen kilometres as the crow flies, from Trier. The earliest phase dates to the middle of the first century and occupation was thereafter continuous until, by north Gallic standards, a very large villa complex stood in the third century (fig. 3.7). Like most villas of the Trier region, the villa was burnt in the second half of that century. Not far from the villa stood a small hillock on which tombs, presumably for the villa owners and family, were found. Below, along the Sauer where a Roman road of some importance (Trier-Reims) crossed the Sauer, extended a cemetery. About the middle of the third century, thus before the burning of the villa, the hilltop was enclosed with a wall of large ashlar stone. In the fourth century the villa continued, but the central building was now fragmented into a number of individual buildings (fig. 3.7). The granary was rebuilt and, strangely, the bath complex enlarged while the dwelling area found itself much smaller than in the previous phases. The central complex did not survive the beginning of the fifth century, after which the only activity which left any trace, was stone robbing.

The enclosure of the hillock was abandoned at the end of the third century (finds are totally lacking) and renewed in the middle of the fourth century. It was furnished with four towers of which the southern one served as a gate-tower. Limestone blocks from a former burial monument seem to have served as a base for a wooden wallwalk. A rectangular building was subsequently added to the interior, and a well. The entirety has been compared with Bitburg and Jünkerath Roman forts, although clearly on a more modest scale (fig. 3.8). Finds are numerous from the late fourth century until the middle of the fifth with a few finds from the second half of the fifth century. The excavators see it as a military station protecting the road.

In 697/8 *villa Epternacus* appears in a charter as Irmina donated her share of it and the villas of Badelingen, Matzen, and Osweiler to the missionary Willibrord, at which time at least one *basilica* and a *monasterium* which the charter tells us Irmina founded herself, are mentioned. These churches were dedicated to the Trinity, Virgin Mary, the saints Peter and Paul, and other

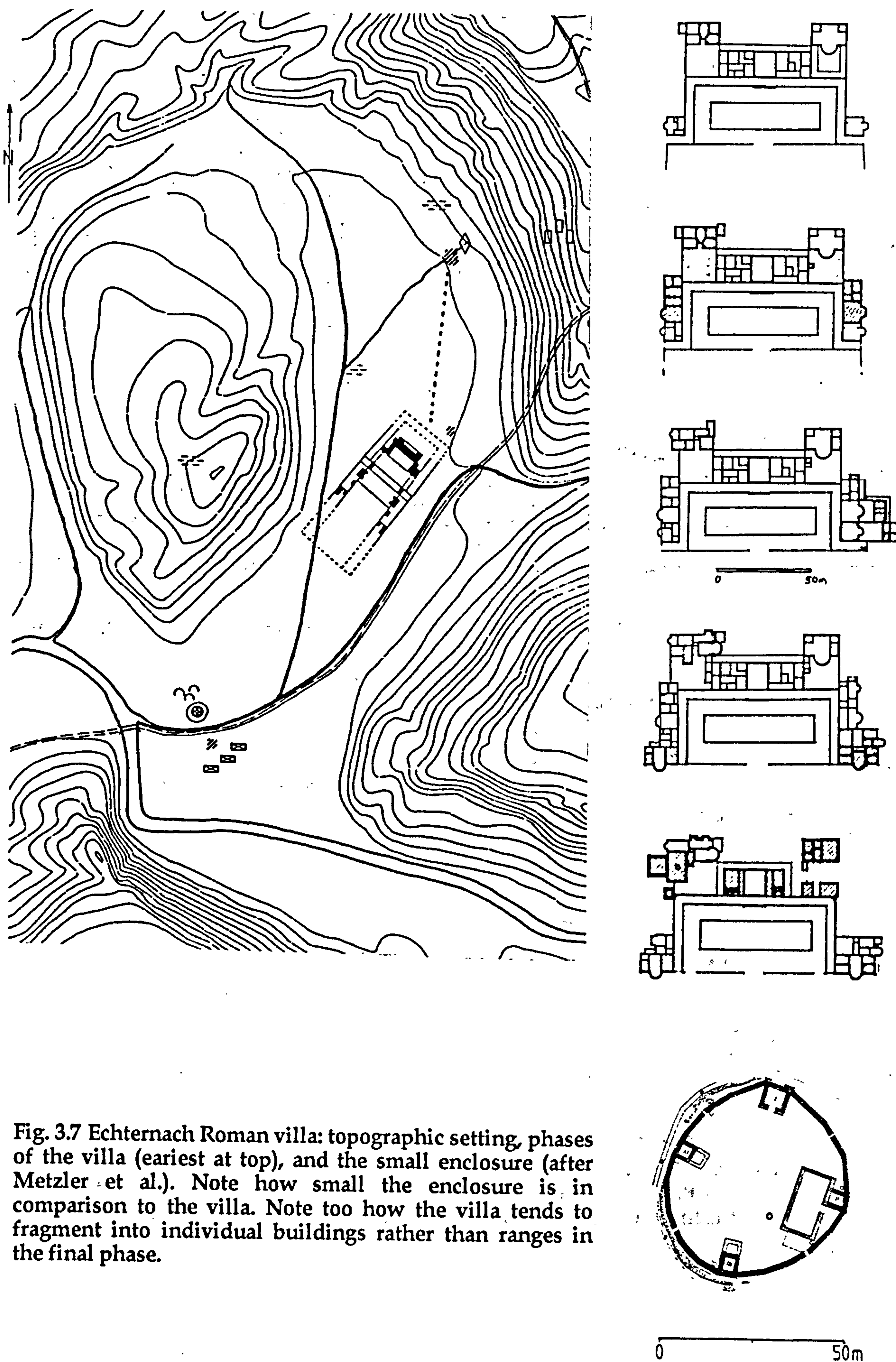


Fig. 3.7 Echternach Roman villa: topographic setting, phases of the villa (earliest at top), and the small enclosure (after Metzler et al.). Note how small the enclosure is in comparison to the villa. Note too how the villa tends to fragment into individual buildings rather than ranges in the final phase.

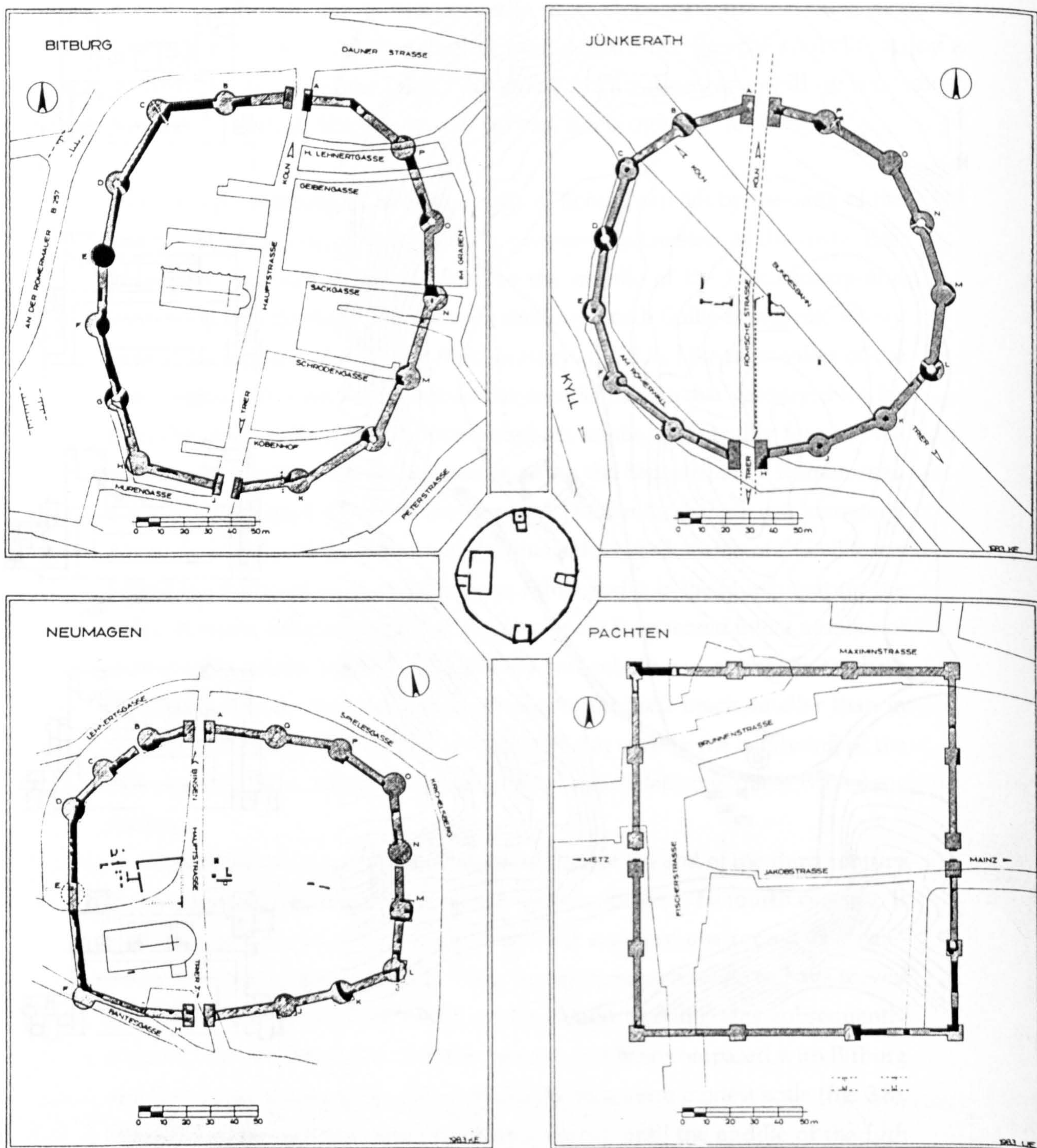


Fig. 3.8 Comparison of the enclosure at Echternach villa with the Roman castra of Bitburg, Jünkerath, Neumagen, and Pachten. Note that the forts dwarf the little enclosure (from Trier Kaiserresidenz und Bischofssitz 1984).

saints. At least one church can be surely identified – the rectangular building set by the west tower of the enclosed hilltop. Over it today (and today it acts as a crypt) lies a Romanesque church dedicated to Peter and Paul. Excavation revealed no Merovingian successor to the Roman building. There is, however, some evidence to suggest that a square 'choir' was added to the northern end of this building. Because Irmina's 'little monastery' was described as attending to wandering monks and the poor, it is postulated that it was a precursor to the St. George hospice mentioned in a charter of 1207.

In 706 Plectrude and her husband Pepin donated a second portion of the *villa Epternacus* to Willibrord's monastery, which he had built on their land. Thus a second monastery was to be found at Epternach and this is plausibly suggested as being located some 100 metres north-west of the enclosed hill. The size and form of the building erected (this time closer to an E-W alignment because not dependent on a previous Roman construction) were remarkably similar to the Peter and Paul church on the hill.

For the sake of completeness we can reconstruct the holding of Willibrord's new monastic complex now, rather than at the end of the chapter which will deal with the extent of Merovingian villas. From Irmina came portions of Echternach, Badelingen, Matzen (?), Osweiler, and later Steinheim. The second half of Echternach from Plectrude and Pepin in 706 and in the next decade, from further Pepinides (Duke Arnulf and Charles Martel), Bollendorf. The various divisions and final reconstruction in Willibrord's hands are seen as representing an original unity (at least of ownership) possessed by Irmina's father Theotar.

Bibliography: Metzler *et al.* 1981; Metzler *et al.* 1983; Bergengruen 1958; M. Werner 1978.

How are we to interpret these facts roughly outlined? To begin, do we need to interpret the enclosed hill as the work of the army? The land was far too close to the villa not to have belonged to it, and was the site of burial tombs interpreted as those of the villa-owning family. The occupation of the hill is not evidenced for the first half of the fourth century. Again this is best explained if it were the private property of the villa owners for a new villa had been built by then on the ruins of the earlier villa. Finally, the hill is clearly the private property of a noble family when it enters the documentary sources at the end of the seventh century. This would imply the continuity of the site's ownership by large landowners. Continuity of occupation, is this a permissible inference? Although the finds-spectrum tails off at the end of the fifth century, I would not take this as evidence

of the end of occupation, for several reasons. Firstly, the dating material par excellence is Argonne samian; its disappearance from the site is clearly not related to the abandonment of the site, but the eventual failure of the pottery. Secondly, the existence of a late seventh-century church built largely (if not entirely) of a late Roman building is assumed without any archaeologically detectable late Merovingian evidence, only documentary. The preservation of the Roman building itself speaks of continued care and up-keep. Finally, there are the similar early, ie. late Antique, St. Peter churches at the Roman *castra* of Jünkerath, Bitburg, and Boppard. On analogy it would seem a likely argument that the N-S oriented Roman building, now St. Peter and Paul's crypt, became a church at least in the fifth century and remained actively used and repaired until its donation to St. Willibrord's monastic foundation. If, in light of the foregoing discussion on the origin of rural churches in the early mediaeval period, the church was a later foundation than the second half of the fourth century, it would be further evidence of its possession by a large landowner and one after the Roman villa had been abandoned. This would imply that the disappearance of the Roman villa of Echternach did not mean the end of the land's integrity as a *fundus*. Now this attempt to 'prove' the existence of a large landowner between that of the late Roman villa owner of the late fourth/beginning fifth century and Theotar in the mid/late seventh century is built of supposition upon supposition. It does, however, fit the evidence better than any interpretation which would see the land divided up among numerous German invaders, before being gathered up again into the hands of a successful royal servant. It would be unwise, doubtless, to claim that Theotar's holding represented the *fundus* as constituted in the last days of the fourth century. It is interesting nevertheless to note that the divisions among Theotar's successors respect the Roman road, the centre of which, dividing the various *villae*, is to be found on the site of the Roman villa at Echternach. This is the sort of proprietary continuity of *fundus*, without the physically survival of the villa buildings, that Agache concluded from his work in Picardie (quoted above).

Quierzy (Coucy-le-Chateau, Aisne). Ten kilometres upstream from Noyon, on the south bank of the Oise, lies the small village of Quierzy, the site of excavation by the German Georg Weise (1923) during the years of occupation in the first World War. The parish church dates only from the middle of the last century, although a report in 1848 mentions the nave of the ruins as being Romanesque. The site (fig. 3.9) was formerly that of a small monastery and a building south of the church, formerly part of the monastic complex, was

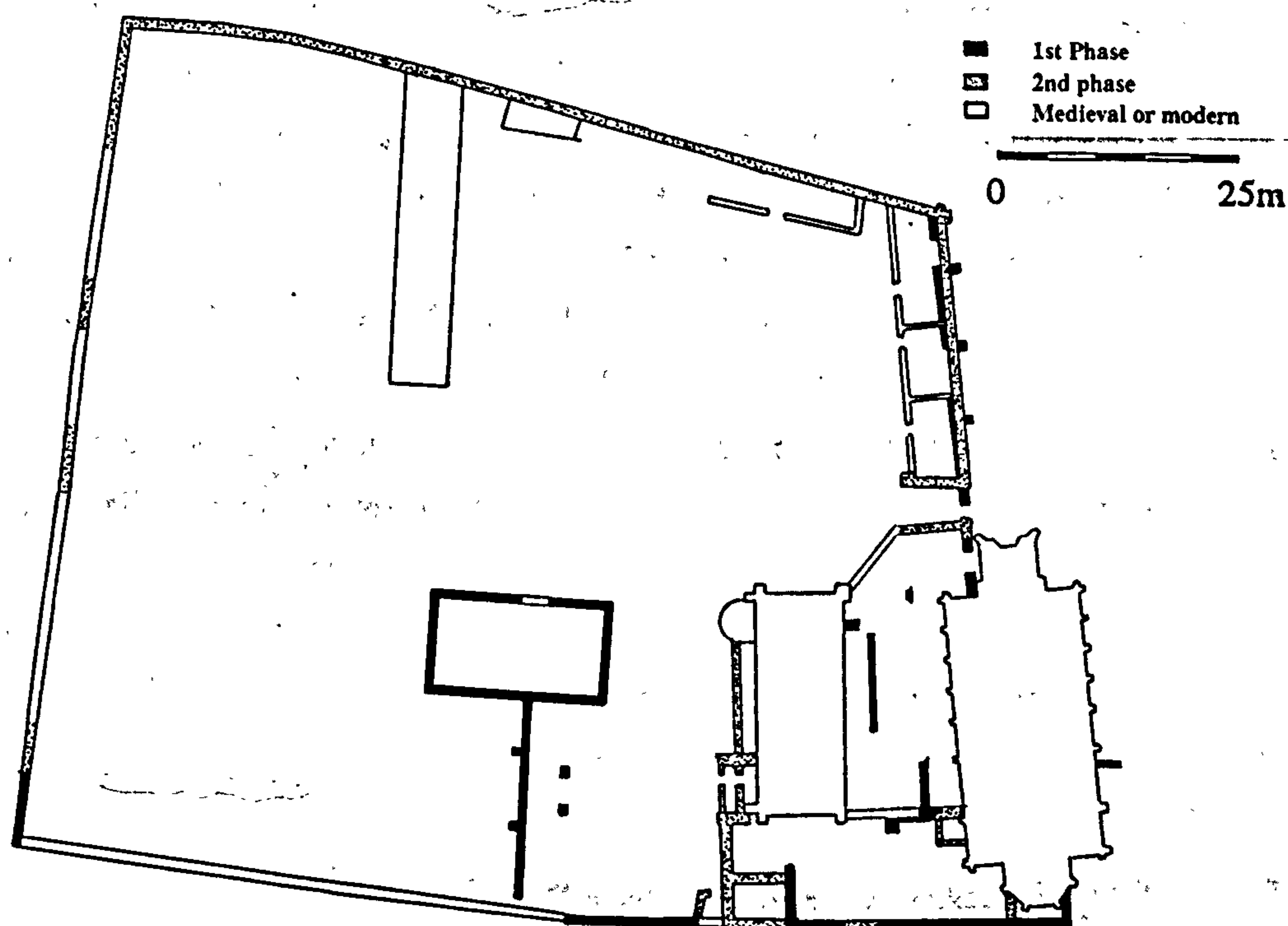


Fig. 3.9 Quierzy, medieval abbey and mortared walls, probably of a modest Roman villa (after Weise). Note that the medieval church obliterates the villa and that the enclosure walls follow the line of part of the villa courtyard walls.

judged by Weise as dating to the beginning of the thirteenth century with major alterations of late Gothic nature. A wall enclosing the monastic complex still exists in parts, and the main gate in early Gothic style is still preserved immediately at the present church's west end.

Underlying the church and Gothic monastery are very clearly the remains of a villa of at least two major phases distinguished by different colours of mortar. Weise tried to date the villa to the earliest Frankish period, and considered it to still be standing in Carolingian times. Oelmann (1923), undoubtedly correctly, saw it as a typical *villa rustica*. Besides the numerous mediaeval pottery sherds were also many late Roman ones and also fewer said by Weise to be of the eighth and ninth century. Weise's attempt to date the villa to something like the fifth century based on the characteristics of the mortar are unconvincing.

What attracted Weise to Quierzy was textual evidence which records that the army of Theuderic camped here in 604/5 facing that of his brother Theudebert, and from 741 until 886 it appears repeatedly as a royal palace of the Carolingians. As such it will appear in a later chapter. Even if we accept that Weise found no architectural remains between those of the late Roman

villa and the Gothic monastery, there still exists an array of evidence for continuity. Ownership of the land by the Merovingians seems probable in 604 and, definitely from 741, Carolingian occupation of some sort is evidenced. In 1068 Philip I made over a *castellum* of Quierzy to the bishop of Noyon (in the meantime a Viking army had camped here at least once) and a church at the latest of the Romanesque period was built here. Now although the present church dates to the last century, it is still oriented on almost exactly the same lines as the Roman villa. But more spectacular is the fact that the monastery complex was enclosed along the exact line of the former Roman *villa rustica* enclosure. Coincidence it is surely not. Can we not exclude the possibility that the Roman ruins naturally suggested themselves to the Gothic monastic community who enclosed their complex along the lines of the villa walls? Although of mortared rough ashlar and measuring 1.1 metres wide, we would then have to imagine that they still stood nearly a millennium uncared for. It is more tempting to believe that the villa was taken over by the Merovingians and later Carolingians who used it to found a monastery.

Tholey (Landkreis St. Wendel, Saarland). In the lower regions of the Hunsrück the abbey at Tholey overlies a luxury Roman villa, following almost precisely the same orientation (fig. 3.10). At the north-eastern end of the Gothic abbey there is a crudely built cell (Steinhaus) in and around which were burials of males without grave-goods, probably monks. The *Testament* of Grimo, made in 632, mentions the abbey at *castrum Teulegio*. Many accept that the villa was still standing (e.g. Böhner 1958) and suggest that the baths under the present-day abbey church were the seventh-century church (Kolling 1973).

Bibliography: Kolling 1973; Levison 1932.

Pfalzel (Trier, Rheinland-Pfalz). Lying on the left bank of the Mosel, some five km. downstream from Trier, Pfalzel sits on one of the few rises which are high enough to avoid flooding. Pfalzel is prominently located, overlooking the major Roman roads which ran along both banks. The Roman building was rectangular, 65 x 56 m., in plan around a central courtyard, with three rooms or towers projecting outward from each side (fig. 3.11). Entrance was gained through a main door in the central tower of the west face, with at least two posterns elsewhere on the ground floor. At least three storeys high, there were no outward facing windows on the ground floor and only small windows on the first floor, while the outward facing windows of the third floor were large. Lighting was achieved by windows opening into the courtyard from the

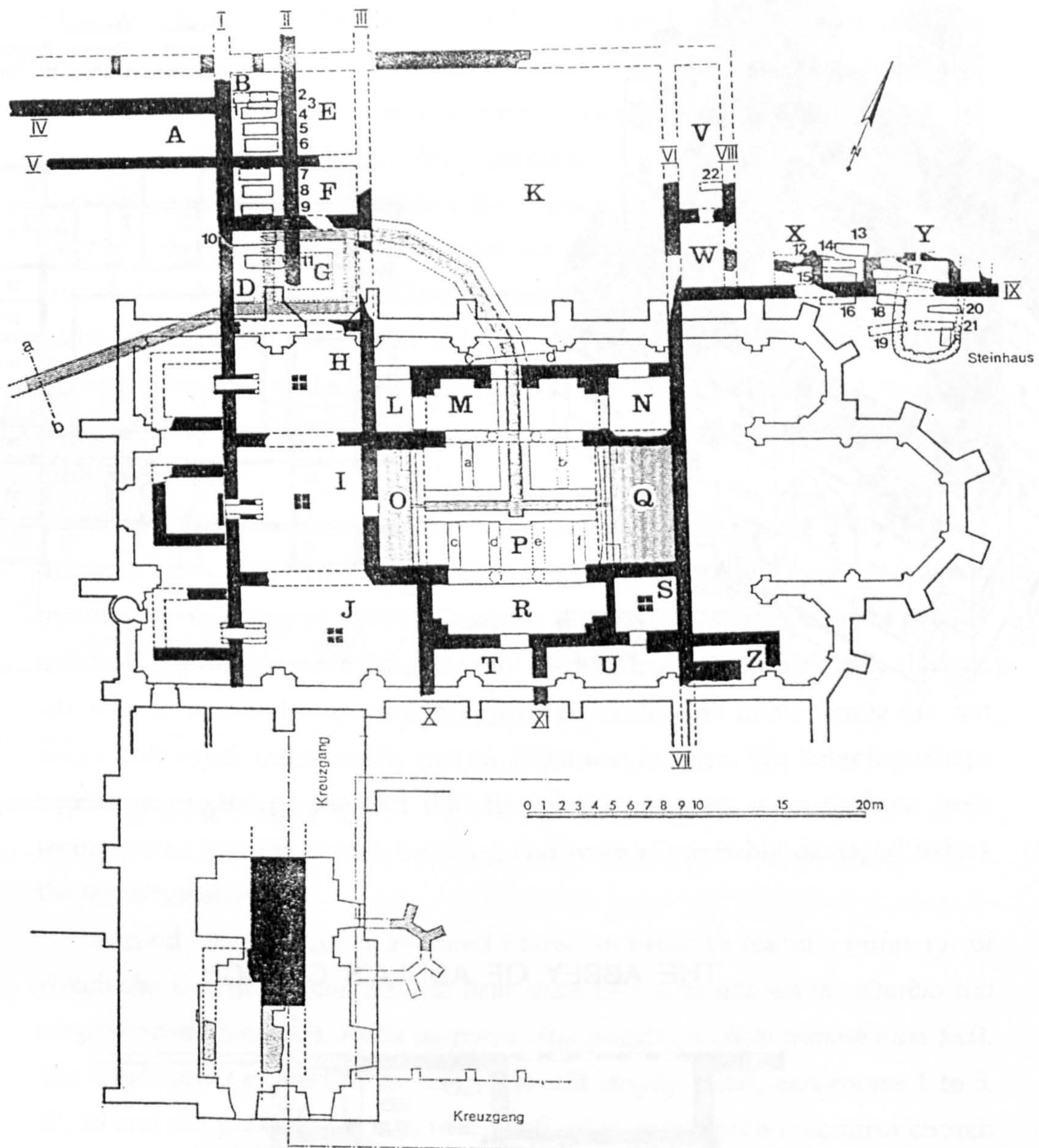
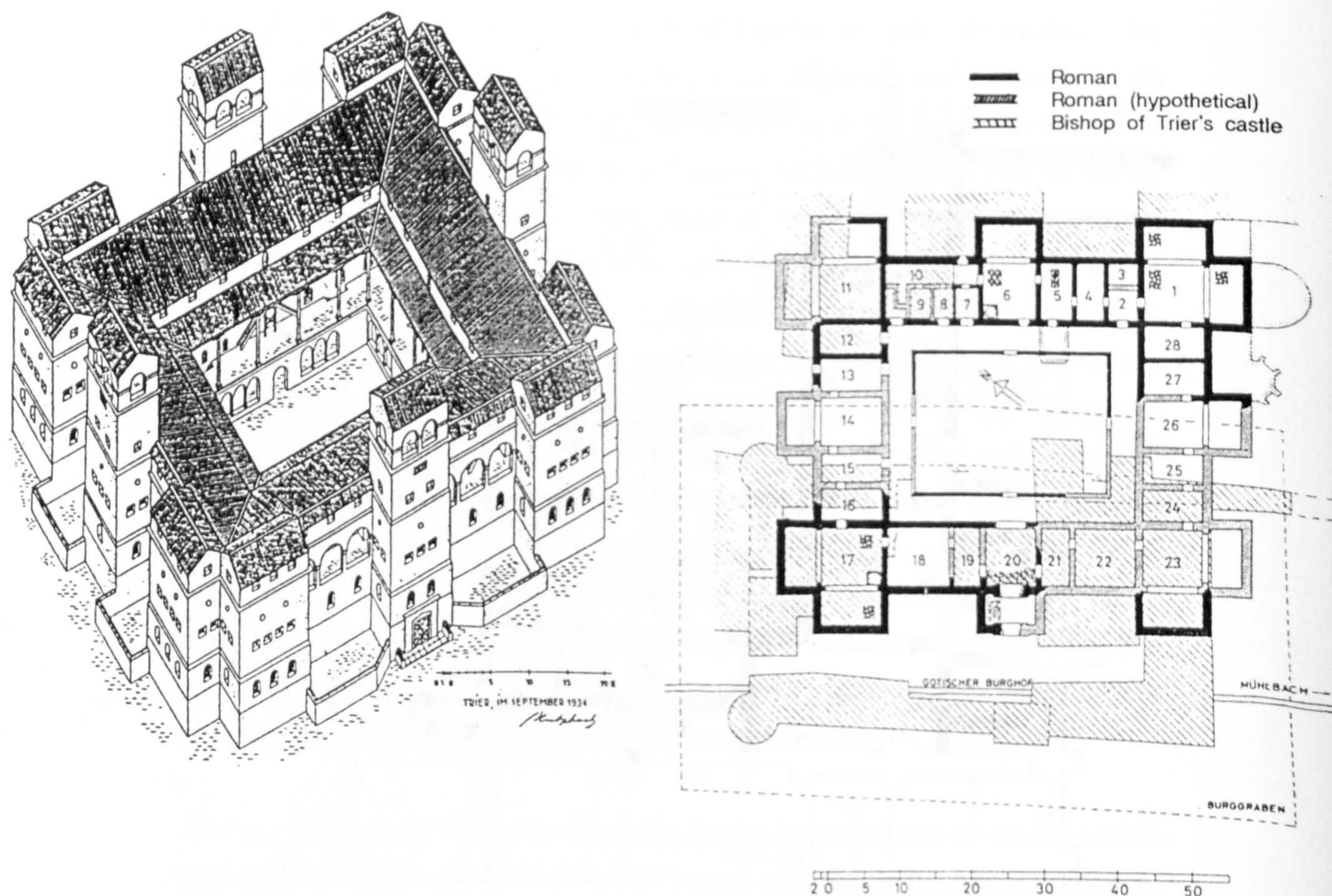


Fig. 3.10 Tholey Roman villa and its relationship to the Gothic abbey.

ground floor rooms. Access to the upper stories was by staircases originating from within the courtyard. Not only were many floors covered in mosaics, but wall mosaics were found on arches of doors and windows and a small bath site was found in rooms 7 to 10. The earliest building phase is dated mid-fourth century and second phase alterations to the latter half of the fourth century. Contemporary with the later alterations was the construction of a similar sized building to the west. Although only a very small portion of the plan has been recovered, the hypothetical reconstruction has been interpreted as barracks.



THE ABBEY OF ADULA'S CHURCH

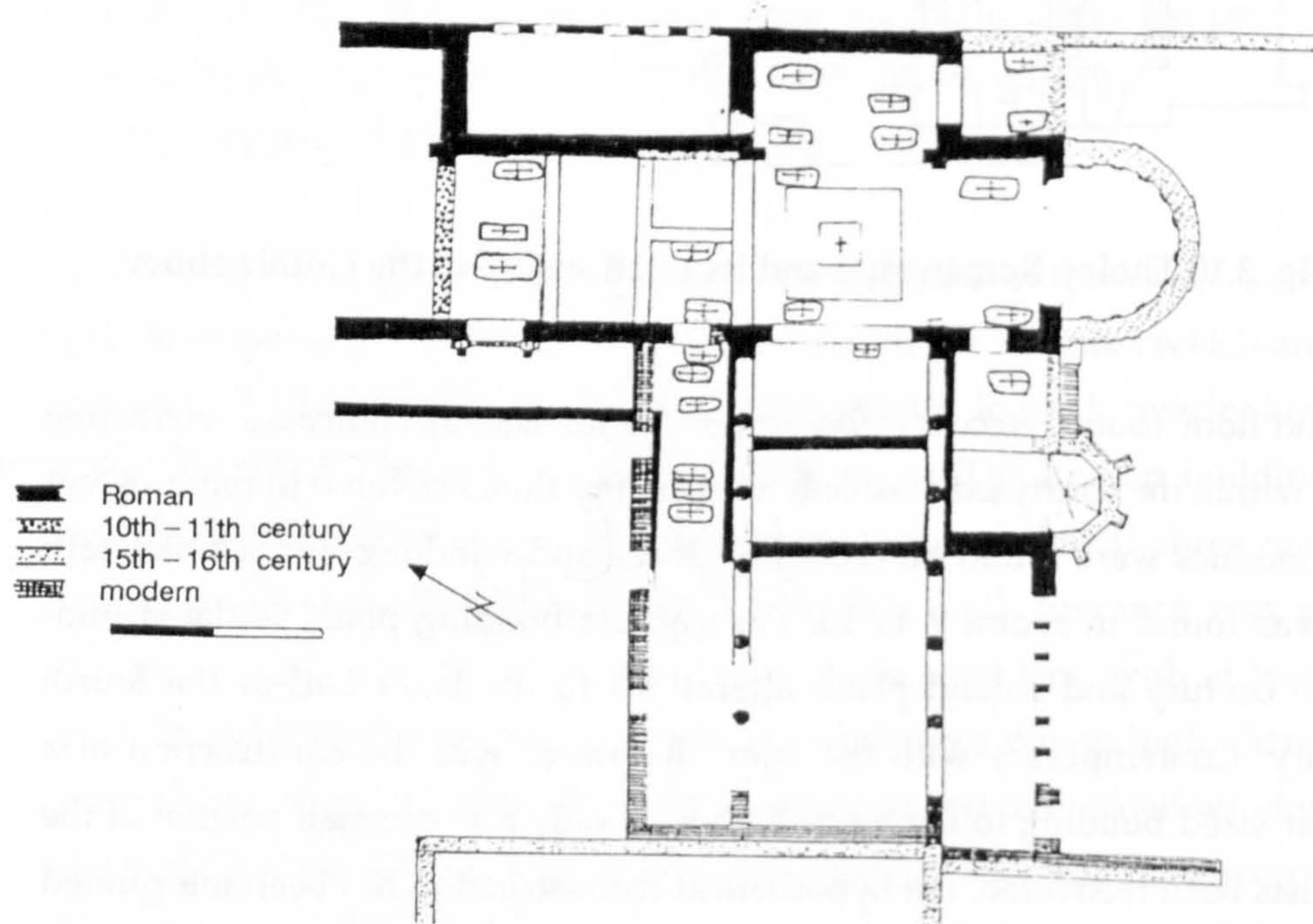


Fig. 3.11 Pfalzel Roman villa: plan, reconstruction, and conversion into a church of St. Adula's nunnery (after Cüppers).

Pfalzel is often considered to have been imperial property. It lies only five kilometres from Trier, the first city of Gaul where there was indisputably an imperial residence, and it lies in the near vicinity of the remarkable Langmauer (see next chapter) which is also thought to have been imperial property. Its name is suggestive: derived from *palatiolum*, was Pfalzel then the 'little imperial palace'? The impressive villa remains at Konz are also suspected of having formed an imperial villa, from documentary evidence. Pfalzel, at any rate, was in the hands of the Carolingians ca. 700, when a charter records the exchange of property between Adula and Pepin of Heristal. The evidence, although circumstantial would seem to indicate that Pfalzel indeed had been imperial property which was taken over by the Merovingian and later Carolingian kings.

How did Palatiolum fare in the hands of the Frankish kings? It was argued by Steinhausen (1957) that lines 25-6 of Venantius Fortunatus' poem *Navigium* mention the building of Pfalzel: *Ducimur hinc fluvio per culmina prisca senatus, Quo patet indiciis ipsa ruina potens*. If this were so it would imply firstly that the site was *senatus*, and thus pertaining to a powerful and noble family but not necessarily royal, and secondly that ca. 590 it was in ruins. The latter is perhaps further suggested by the fact that the mosaics do not seem to have been incorporated into the church building and were presumably damaged before the transformation.

Around 700 AD, Adula obtained Pfalzel in order to found a nunnery, of which she was to become abbess: *item aliam in palacio antiquo in suburbio sito congregationem constituit. Adela autem in villa palaciolum dicta monasterium fecit*. The west corner of the Roman villa was still largely intact, and rooms 1 to 5, 27, 28 and the projecting wings were utilised to construct a cruciform church without any addition being necessary. The dividing wall between rooms 1 and 28 and between 2, 3 and 4 were knocked down (fig. 3.11). The remaining sections of the villa are presumed to have been adapted to the nunnery's needs. The eastern corner of the villa survived into the sixteenth century as part of the bishop's residence.

Bibliography: Cüppers 1962; 1964; 1965; Kutzbach 1935; Steinhausen 1957; Trier Kaiserresidenz und Bischofssitz 1984; Wightman 1971.

The foregoing four examples were all chosen because they are documented as Carolingian villas and yield archaeological evidence of a Roman villa from which they are presumed to have descended. The documentary evidence, almost by definition, reveals churches at all three. Yet all are quite intractable when we

demand of them conclusive archaeological proof of continuity through the fifth century and Merovingian period; the early mediaeval settlements and churches are barely visible archaeologically. At Quierzy we could easily believe that the cause lay in the excavation techniques: only small trenches were opened and usually only to follow the traces of stone walling already discovered, thus unsuitable for recognising the remains of wooden constructions. But the other two sites tell us that the reasons are definitely deeper. At Echternach mid- and probably late fifth-century occupation of the St. Peter's church hill is supposed, but proof is not forthcoming. The fourth-century towers and rectangular building that were subsequently to become St. Peter's church are likely to have been occupied, although occupation levels were not recognised. At both Pfalz and Echternach we are fully positive that we have found Roman structures which were converted into churches in the early eighth and mid- to late seventh century respectively; at Tholey we can only suspect they were utilised in the early seventh century. In neither of the former two have archaeologists discovered the slightest hint of construction or near-by occupation; at Tholey Carolingian pottery was found (Kolling 1973). Without the documents we would never have hypothesised or probably even have suspected such a date, for excavation under normal circumstances has yielded no clues whatsoever. This phenomenon will appear time and again in the early mediaeval period. It makes it clear that all arguments from negative evidence will need be guarded against.

One can grasp the problem of archaeological invisibility somewhat better, if one thinks about early mediaeval churches and how much is known about their architecture. Syntheses of Merovingian ecclesiastical architecture do not immediately spring to mind. The catalogue *Vorromanische Kirchenbauten* (Oswald *et al.* 1966) may do so, but most of the entries are Carolingian or Ottonian. It may be that the poorer quality of the masonry or even building in timber has made these early churches difficult to spot. Where the best evidence does exist, in Switzerland (Sennhauser 1979), the 'complete' plans give the impression of rather small edifices. This picture is reinforced by the excavation of small *Eigenkirchen* that are becoming increasingly common in excavations of row-grave cemeteries. However, it should not be overlooked that such churches, known from excavation, tend not to be the sites of major early mediaeval monastic or episcopal centres. Of Merovingian cathedrals, Fehring (1957, 87) notes, we have only recently begun to learn anything, and then all those he quotes are in Germany. In Gaul itself there is only patchy evidence from Lyon, Fréjus, Digne, Aix-en-Provence, Thérouanne, and now forthcoming, Rouen.

Yet elusive as these early churches are, they can occasionally be found built on

top of Roman villas. Martin (1979) documents several Swiss examples. At Messen the seventh-century 'founder's' grave overlies a segment of an earlier church built on a Roman *villa rustica*. At Ardon an early church incorporates a late Roman crypt lying near and inside the walls of a villa enclosure. The seventh-century graves at Meikirch lie inside and oriented along the walls of a Roman villa, some of which are assumed to have been used in the church's construction. At least this would explain the meagre remains of contemporary walling assumed to have formed the church in which the burials lay. At Laupersdorf the sixth- or seventh-century church was found complete, with burials, probably within the domestic range of the Roman villa. An early mediaeval timber-built church has been excavated within Roman villa ruins at Satigny (Bonnet 1977). The sixth-century church within the Roman villa at Commugny relates so well to the whole that it is tempting to see the villa as still standing (fig. 3.6).

While we are tempted to place a church in Echternach in the fifth century, it is striking that we otherwise find a sixth, seventh, and eighth-century date more common for the earliest recognisable churches appearing on top of or amidst the architectural remains of Roman villas. The large Roman villa of Mienne (Eure-et-Loire) can be dated by its mosaics as having survived into the fifth century. Excavation early last century was not of the quality to allow us to draw conclusions about subsequent occupation, but white marble capitals deriving from the dig are perhaps of the seventh century; one presumes from a Merovingian church (but a Merovingian secular construction is quite possible) (Heitz 1985, 194). As patchy as the archaeological evidence is, it supports the now very old hypothesis of Imbart de la Tour that the development of important churches on villa estates was a continual process from the fifth century to the Carolingian period and by no means commoner early than later. And therefore, if Roman villas survived the fifth century into the early Middle Ages, and if they were eventually to give rise to parish churches or monasteries, it was not first and foremost this ecclesiastical function or ownership that allowed that survival. It must have been their agricultural and social position.

Roman Remains under Frankish Villas

In the case of Echternach, Quierzy, Tholey, and Pfalzel we have Roman villas documented archaeologically and Frankish villas documented textually. Most of the discussion of fifth-century continuity of villas, in English at any rate, concentrates on the archaeological record of Roman villas. Interest in working backwards, as it were, from textually documented Merovingian and Carolingian villas, looking for evidence of previous Roman occupation, remains largely the

domain of French local historians and archaeologists.

This area of research has been too long neglected and the results of local investigations cannot be accepted at face value. The tendency is to take any trifling Roman artefact from a wide radius around a village documented in a Frankish text as evidence of a previous Roman villa. Most important sites will be discussed in the appropriate later chapters, but here we might give an example of work done on the royal villas of the Ardennes by Helga Müller-Kehlen (1973). I have summarised her findings in table 3.1, but used my own judgement to rate the importance of the finds she discusses.

| Modern name | Documented name | First mention | royal visit | Roman finds | Merovingian finds |
|--------------|---------------------------|---------------|-------------|-------------|-------------------|
| Amberloup | <i>Ambarlao villa</i> | 888 | 896 | XXX | XXX |
| Amblève | <i>Amblava curtis</i> | 670 | XXX | X | |
| Baelen | <i>Bailus villa</i> | 888 | X | | |
| Bastogne | <i>Bastoneco</i> | 634 | XXX | | |
| Besslingen | <i>Belsonancum villa</i> | 585 | 585 | XXX | XXX |
| Büllingen | <i>Bulinge villa</i> | 888 | 940 | X | |
| Chassepierre | <i>Casapetrea villa</i> | 888 | X | | |
| Cherain | <i>Charancio curtis</i> | 670 | X | | |
| Chevigny | <i>Kavineiacus fiscus</i> | 840? | XXX | | |
| Glains | <i>Glamau villa</i> | 720? | 720? | X | |
| Jamoigne | <i>Gammunias villa</i> | 888 | XXX | XXX | |
| Konzen | <i>Compendio villa</i> | 888 | X | | |
| Lierneux | <i>Ledernao curtis</i> | 670 | X | | |
| Longlier | <i>Longolario</i> | c.620 | 620... | X | XXX |
| Mellier | <i>Maslario palatium</i> | 763 | 763 | | |
| Neundorf | <i>Novavilla villa</i> | 888 | X | | |
| Orgeo | <i>Uriaus fiscus</i> | 879 | 879 | | |
| Ortho | <i>Ortao villa</i> | 888 | XXX | X | |
| Paliseul | <i>Palatiolo villa</i> | 747? | 897 | X | |
| Rechain | <i>Richeim villa</i> | 888 | X | | |
| Sprimont | <i>Sprismonte villa</i> | 888 | X | | |
| Theux | <i>Tectis fiscus</i> | 814 | 820 | XXX | X |
| Thommen | <i>Tumbas fiscus</i> | 814 | 844 | X | X |
| Walhorn | <i>Harna villa</i> | 888 | X | | |

Table 3.1 Royal Carolingian estates in the Ardennes. Important or numerous Roman and Merovingian finds from the vicinity are marked XXX, few stray finds marked X (compiled after information from Müller-Kehlen 1973)

Literary Evidence and the Architecture of Fifth-Century Villas

Perhaps one of the few incontrovertible remarks we can make about post-Roman villas is that they were not invisible. Percival offers the hypothesis that two possible developments occurred in the transformation of late Roman villas; fortification and nucleation. These were perhaps the physical expressions of the

late Roman tendency for people to attach themselves to prominent landowners (Percival 1976, 177). He suggests that this physical change is partially responsible for the general archaeological invisibility of villas in the fifth and subsequent centuries – we are looking for the wrong thing. To date, documentary evidence still gives us the best impression of what fifth-century villas may have looked like, so it is here we must start. Evidence from mere snippets has generally been overlooked, although there is much to be gained. Here, for example, is Paulinus of Pella (*Eucharisticos* 204-13), writing early in the fifth century:

[Eventually my concern was for luxury.]
 I wanted an elegant house with spacious rooms
 and suited to the varied seasons of the year;
 My own table was richly and handsomely set;
 my servants were not only young but numerous;
 the place was furnished with taste and variety;
 the silver was more distinguished for its value than weight;
 many skilled workmen were there to fill my requests;
 many well-bred, well-trained horses filled my stables
 and there were carriages to take me where I wished.

But the traditional starting point is Sidonius Apollinaris and five villas which appear in his poems and letters: Avitacum, his own property through his wife the daughter of Avitus (*Epist.* 2.2); a villa of Consentius near Narbonne (*Epist.* 8.4); Vorocingus and Prusianum; and Burgus, a villa of Pontius Leontius (*Carmina* 22)(translations from Anderson 1965):

To Domitius.

[After an introduction, Sidonius invites Domitius to the coolness of his country house.] 3. Just let me tell you, if you don't mind, how this country place you are invited to is situated. We are at Avitacum; this is the name of the farm (*praedio*), which is dearer to me than the property I inherited from my father, because it came to me with my wife: such is the harmony in which, under God's guidance, I live with my family (I hope you are not afraid of the evil eye!). On the western side is a mountain, earthy in substance but stiff to climb, which pushes out lower hills from itself like offshoots from a double stem; and these hills diverge so as to leave a breadth of about four *iugera* between them. But before spreading out so as to allow a sufficiently large frontage for a dwelling, the hillsides escort the intervening valley in straight lines, right up to the outskirts of the mansion (*villae*), which has its fronts facing north and south. 4. On the south-west side are the baths (*balineum*), hugging the base of a wooded cliff, and when along the ridge the branches of light wood are lopped, they slide almost of themselves in falling heaps into the mouth of the furnace. At this point there stands the hot bath (*cella coctilium*), and this is of the same size as the anointing-room (*unguentariae*) which adjoins it, except that it has a semicircular end with a roomy bathing-tub, in which part a supply of hot water meanders sobbingly through a labyrinth of leaden pipes that pierce the wall. Within the heated chamber there is full day and such an abundance of enclosed light as forces all modest persons to feel themselves something more than naked. 5. Next to this the cold room (*frigidaria*) spreads out; it might without impertinence challenge comparison with baths (*piscinas publicis*) built

as public undertakings. First of all the architect has given it a peaked roof of conical shape; the four faces of this erection are covered at the corners where they join by hollow tiles, between which rows of flat tiles are set, and the bath-chamber itself has its area perfectly adjusted by the nicest measurements so as to find room for as many chairs as the semicircular bath usually admits bathers, without causing the servants to get in one another's way. The architect has also set a pair of windows, one opposite the other, where the vaulting joins the wall, so as to disclose to the view of guests as they look up the cunningly-wrought coffered ceiling. The inner face of the walls is content with the plain whiteness of polished concrete. 6. Here no disgraceful tale is exposed by the nude beauty of painted figures . . . 7. In short, there will not be found traced on those spaces anything which it would be more proper not to look at; only a few lines of verse will cause the new-comer to stop and read: these strike the happy mean, for although they inspire no longing to read them again, they can be read through without boredom. If you ask what I have to show in the way of marble, it is true that Paros, Carystos and Proconnesos, Phrygians, Numidians and Spartans have not deposited here slabs from hill-faces in many colours, nor do any stone surfaces, stained with a natural tinge among the Ethiopian crags with their purple precipices, furnish a counterfeit imitation of sprinkled bran. But although I am not enriched by the chill starkness of foreign rocks, still my buildings - call them cottages (*tuguria*) or huts (*mapalia*) as you please - have their native coolness. However, I want you to hear what we have rather than what we have not. 8. Attached to this hall is an external appendage on the east side, a *piscina*, or, if you prefer the Greek word, a *baptisterium*, which holds about 20,000 *modii* (approx. 40,000 gallons). Those who come out of the heat after the bath find a triple entrance thrown open to them in the centre of the wall, with separate archways. The middle supports are not pillars but columns, of the kind that high-class architects have called 'purples'. A stream is 'enticed from the brow' of the mountain, and diverted through conduits which are carried round the outer sides of the swimming-bath (*natatoriae*); it pours its waters into the pool from six projecting pipes with representations of lions' heads: to those who enter unprepared they will give the impression of real rows of teeth, genuine wildness in the eyes and unmistakable manes upon the neck. 9. If the owner is surrounded here by a crowd of his own people or of visitors, so difficult is it to exchange words intelligibly, owing to the roar of the falling stream, that the company talk right into each other's ears; and so a perfectly open conversation, overpowered by this din from without, takes on an absurd air of secrecy. On leaving this place one comes across the front of the ladies' dining-room (*triclinii matronalis*); joined on to this with only a barrack partition between them, is the household store-room (*cella penaria*), next to which is the weaving-room (*textrina*). 10. On the east a portico overlooks the lake; it is supported on round composite pillars rather than by a pretentious array of monolithic columns. On the side of the vestibule extends inward a length of covered passage - covered but open, being unbroken by partitions; this corridor has no view of its own, so, although it cannot claim to be a hypodrome, at any rate I am entitled to call it a crypt-portico. At the end of this passage, however, a part is stolen from it to form a very cool chamber, where a chattering crowd of female dependants and nursemaids spread a feast for the gods, but sound the retreat when I and my family have set out for our bedrooms (*dormitorium cubiculum*). 11. From the crypt-portico we come to the winter dining-room (*hiemale triclinium*), which the fire often called into life within the vaulted fireplace has stained with black soot . . . From this dining-room we pass to a living-room or small dining-room (*diactam sive cenatiunculam*), all of which lies open to the lake and to which almost the whole lake lies open. In this room are a semicircular dining-couch and a glittering sideboard, and on to the floor or platform on which they stand there is a gentle ascent from the portico by steps which are not made either short or

narrow. Reclining in this place, you are engrossed by the pleasures of the view whenever you are not busy with the meal. 13. When you have finished your meal, a drawing-room (*deversorium*) will offer you welcome, one which is truly a summer room because it is not in the least sun-baked, for, as it is open to the north only, it admits daylight but not sunshine; before you reach it there is a narrow ante-chamber, where the somnolence of the ushers has room to doze rather than to sleep. [Remainder devoted to describing the lake-side and games that await Domitius]

To Consentius.

My honoured lord, will the Octavian property of yours ever, by God's good pleasure, see us united? It is indeed not so much your property as the property of your friends. Close to the city (Narbonne), the river (Aude), and the sea, it feeds your guests with feasts and you with guests; moreover, its lay-out charms the eye of the beholder. In the first place the house rises high, with walls skilfully arranged so as to produce an undoubted architectural symmetry. Again, it sends forth a gleam far and wide from the chapel, the colonnades and the baths, which are all conspicuous. In addition, its fields and springs, vineyards and olive-groves, its entrance-court (*vestibulo campo*), its park, its hill present a most lovely view. Then, besides a well-stocked larder and abundant furniture, it is liberally filled with stores of books, amid which you expend as much energy on the pen as you give to the ploughshare, so that it is hard to decide whether the owner's land or his mind has been the better cultivated.

To Donidius

... I have spent the most delicious time in visiting two charming properties and two most sympathetic hosts, Ferreolus and Apollinaris. Their estates have a common boundary, and their residences are near, being connected by a road which is long enough to tire the pedestrian but hardly long enough for a ride. The hills which rise above the buildings are cultivated by the vine-dresser and the olive-grower . . . One house has a view over flat and open ground, the other looks out on woods; yet though they differ in their situation they are alike in their charm. But why should I say more of the lie of the farms when there remains to be disclosed the whole scheme of my entertainment? . . . Each morning saw the start of a really charming contest between the two parties about their guest, to decide which of the two kitchens should be the earlier to steam with my meal . . . Well, I was hurried from bliss to bliss. Hardly had I entered one vestibule or the other when behold! I found on one side opposing ball-players bending low amid the whirling evolutions of the *catastrophae*; in another quarter I would hear the clatter of rattling dice-boxes and of dice mingled with the rival shouts of the gamesters; in another part were books in any number ready to hand; you might have imagined yourself looking at the shelves of a professional scholar or at the tiers in the Athenaeum or at the towering presses of the booksellers. The arrangement was such that the manuscripts near the ladies' seats were of a devotional type, while those among the gentlemen's benches were works distinguished by the grandeur of Latin eloquence [he then proceeds to describe the books and literary discussions]. While all and sundry occupied themselves in these pursuits according to their individual tastes, a messenger would approach from the head cook to tell us that the time for refreshment was at hand. He had his eye on the passage of the hours as marked by the water-clock, and as the fifth hour was just departing he was proved to have arrived just at the right moment. The luncheon was at once short and lavish, in the style of senators, who have an inherited and established practice of having abundant viands served up on few dishes, although the meal is varied by having some of the meats roasted

and others stewed. As we sat over our wine there were short stories, for amusement or instruction; they were started in two sets, bringing mirth and edification respectively. To sum up, our entertainment was moral, elegant, and profuse. We then rose from table, and if we were at Vorocingus (this was the name of one of the estates) we returned to our baggage and our lodging; if we were at Prusianum (so the other property was called) we turned out of their beds. Tonantius and his brothers, the flower of all the young nobles of their age, because it was not easy to carry our own sleeping-kit so often from place to place. After shaking off the midday drowsiness we took short rides to whet our appetites, jaded with eating, to the keenness needful for dinner. Both my entertainers had baths in course of erection; in neither case were they in working order. However, when the convivial crowd consisting of my attendants and the household servants, whose heads the hospitable bowl was wont to souse and overpower, had left off drinking, at least for the moment, a trench would be hastily dug close to the spring or the river, and a pile of heated stones poured into it. Then while the ditch was heating it was roofed over with a dome constructed of pliant hazel twigs twined into a hemispherical shape; in addition, rugs of hair-cloth were thrown over this roof, shutting out the light and darkening the open spaces between the twigs, so as to keep in the rising steam which is created by pouring boiling water on hot stones. Here we whiled away the hours with no lack of witty and humorous conversation, in the course of which we became wrapped and choked in the breath of the hissing mist, which drew forth a wholesome perspiration. When this had poured out sufficiently to please us we plunged into the hot water. Its kindly warmth relaxed us and cleared our clogged digestions, and then we braced ourselves in turn with the cold water of the spring and the well or in the full flow of the river; for I should explain that the river Gard flows midway between the houses.

Burgus of Pontius Leontius

Stranger, whoever you may be, that have visited Burgus and yet are fain to keep silence about it . . . whoever you are who, with no praise on your lips, view that splendid home, you are thereby put on view yourself; your inclination loudly heralds itself though without voice, for your silence proclaims you dumb with jealousy.

. . . There is a place where two rivers, the Garonne, sped whirling down from a dripping mountain-crag, and the mossy Dordogne, which rushes with like swoop to the plain and at last flows out from a bend in its sandy channel, gradually commingle their slowing streams. Here the sea rushes up against the current and with constant coming and going repels or courts the waters that the rivers roll down . . . Between these rivers, but nearer to one than to the other, there is a mountain piercing the sky, conspicuous in its towering height but destined to have owners still more elevated and to be the birthplace of senators. Some day, when his land shall be under Latin sway, Paulinus Pontius, the founder of the family, shall surround that hill with walls (*moenibus*), and the towers (*turres*) shall soar beyond earth's atmosphere; thus on their summits shall rest, shining with a common radiance, the two lights of Stateliness and Succour. Those walls (*muros*) no engine, no battering-ram, no high-piled structure or near-built mound, no catapult hurling the hissing stones, no tortois-roof, no mantelet, no wheel rushing onwards with ladders already in position shall ever have power to shake. Methinks I see the future that is in store for thee, O Burgus (for so thou shalt be called). The house (*domus*) rises from the river's brim and gleaming baths are set within the circuit of the battlements (*propugnacula*): here when the surging waters are troubled by the murky north-wind, the eaten, jagged rock sends forth a roar from the scarred bank; then from a cleft in the crags a torrent leaps forth and is

shot aloft, showering spray on to the very roofs; it lifts up men in boats and often mocks them with a sportive shipwreck; for when the storm is over the flood retreats and strands whole fleets that have been forced up into the baths. But the columns that support the baths, of what manner and size are they? Before them must bow the costly dark hue in the purple quarry of Synnada and the Numidian hill that bears stones like ivory and the marble that burgeons with grass-like veins; henceforth I spurn gleaming Paros and Carystos; poorer now seems the purple suspended in the blushing rock.

Lest posterity should be uncertain whom the building boast as its stablisher, a stone is set in the ground at the entrance (*in introitu lapis*) with the names of the founders clearly graven upon it; and there is water near at hand which clears away all footprints and wipes of all mud with its flooding stream. The house-wall is faced with slabs of cut marble up to the gilded ceiling, which is right fitly concealed by the yellow metal, for the rich prosperity of the house, brooking no secrecy, reveals its wealth when thus it hides its roof. Behind this part there soars, passing high above a double floor, a colonnade likewise double, unknown to the double Wain. This again diverges gently backward, and finally these curving wings turn their horns inward for a little way, and so look back upon it. Its right bend sees the dawn, its front the noon-day light, its left the fading day. It loses none of these three quarters of the heavens, but preserves the whole of the sun in the crescent hall. (Three paintings then described).

Higher up the granaries (*horrea*) multiply with their long stretch of buildings and with produce within so abundant that even their vast space is cramped. Hither shall come as great a harvest as is reaped in Africa's warm fields . . . Then there is a summer portico exposed on one side to the chill north: at the other end a harmless warmth comes out from the winter baths and tempers the air of the place when the season requires; so this end is best suited to the cold weather; for the part that fights shy of the Lion's mouth [ie. July] is thereby unfitted to endure the rage of Lycaon's Bear. Into the warm baths of the mansion (*arcis*) comes a stream from far above, which falls into the mountain, being forced through open channels till at last it circulates its waters under cover through divergent tunnels. Behind the shaded granaries there rises toward the west a structure that is the winter home of the master and mistress (*opaca quae dominis hiberna domus*); here a goodly fire crackles, which devours the great logs that are piled near at hand; the glowing cloud that comes forth in billows curls upward from the stove, then fades away, and with its blast now broken it spreads a mitigated heat all over the roof. Joined to the room may be seen the weaving-chambers (*textrina*), which the founder dared to build in a style that vied with the temples of Pallas. Some day it shall be blazoned forth by fame that in this sanctuary the worshipful lady of the great Leontius, than whom no other wife of the Pontian house ever rejoiced more in her husband's illustrious rank, stripped the Syrian distaff and twisted the silken strands along the light reeds and spun the pliant metal, making the spindle swell with thread of gold . . .

You turn left, and a spacious colonnade receives you, its shape curved but its passages straight. To the extreme edge clings a crowded forest of close-set columns. Here is built a lofty dining-room (*alta volubilibus*) with folding-doors. A conduit of cast metal is near; there is a suspended tank in front of the door: into it the water falls from above, and fishes, advancing with the flow, find the end of their swimming in an upper room – but a watery one. Close at hand rises the first, or, if it please you better, the last of the towers (*vel prima vel extrema turris*). There the masters of the house will be wont to set their dining-couch (*sigma*) in winter. Often-times on its far-seen roof will I sit and view that mountain beloved by my Muses and by the goats; I will walk amid those laurel boughs, and there I shall believe that the timorous Daphne believes in me. Then if you chance to turn your step towards the two Bears to reach the

temple of that God who is greatest of all, you find the wine-store and the larder (*apothec penusque*) fragrant with mingled delights. This place will see much of you, my brother.

Finally, there is the textual evidence from the inscription at Theopolis, lost in the upper reaches of the Jabron, a tributary of the river Durance, in Provence (fig. 3.12). Despite its lofty name, with its city pretensions, and the humble epithet of *locus* on the stone, we are probably best advised to think of Theopolis as a villa.

Theopolis (Alpes-de-Haute-Provence): An inscription in living rock, long-known to scholars (CIL 12, 1524), commemorates what one assumes to be the establishment of Theopolis by Claudius Postumus Dardanus and his wife Naevia Galla. Dardanus was the praetorian prefect of Gaul in 409, 411, 412, and possibly 413 AD and is best known for having defeated and executed Jovinus (for his career see Matthews 1975). The chosen name of the *locus*, an unusual one, is most plausibly connected with the great work of St. Augustine, as Dardanus was a correspondent of both St. Jerome and St. Augustine. The description of the site given on the inscription is of great interest:

... in the place named Theopolis, (Dardanus and his wife) provided the use of roads by cutting (?) on both sides of the mountain; they gave walls and gates [structures] which, established on their own land, they wished to be for the communal safety of all. ...

Our incredulity must be stretched to its limits when we are told that the site itself cannot be found, for no subsequent occupation has greatly disturbed this remote area and the inscription is very clearly *in situ*! Cause for doubt in the minds of modern scholars must be linked with the numerous clues which the countryside furnishes. The following localities have all been the objects of investigation: the farm of Theous, the chapel of Notre Dame de Dromon, the village of St. Geniez and a terrace called 'les Planeaux'. Because of its name, Theous has been suggested as the descendant of Theopolis although on philological grounds the name could not descend directly and supportive arguments become involved. Decreasing credibility further, the farm is set in less fertile lands some eight or nine kilometres from the inscription further up into the hills. The chapel of Notre Dame de Dromon, some six or seven kilometres from the inscription has caught the popular imagination, not least because of its present isolation. One romantic interpretation of Theopolis is that it was a monastery to which Dardanus retired (Châtillon 1943). This chapel clearly appeals to such an interpretation. Containing a 'maladroit and late' ram's head capital of local stone, it has been suggested that a fourth- to

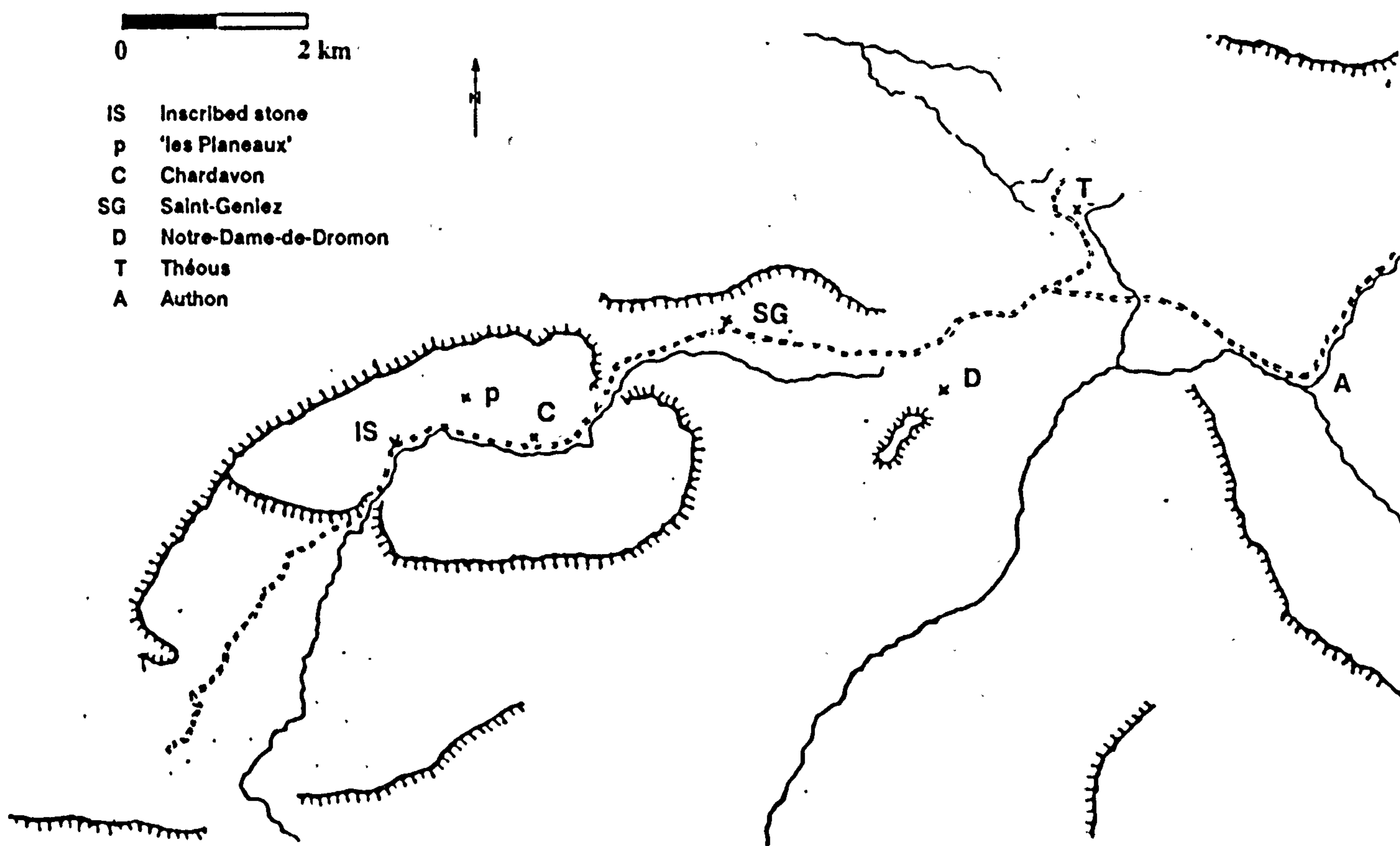


Fig. 3.12 Theopolis (after Marrou).

sixth-century example was used as a model, such as an example found in Arles. The crypt has been dated to the eighth or ninth century by Benoit, although Marrou would like to see the capitals, on which much of the dating argument rests, as later refurbishments (Marrou 1954, 106-8). Wishful thinking is at work here, raising the possibility that the crypt of the chapel could be the *martyrium* of Dardanus. Perhaps the theory is best left as possible, but improbable. St. Geniez is of interest because the saint celebrated in its name was particularly venerated in Arles and thus forms a possible connection with Dardanus as praetorian prefect operating from Arles. Even if this tenuous link could be proven, it would not pinpoint the site of Theopolis.

Surely the most plausible initial theory would be that the inscription marks the actual site of Theopolis, if not indeed its entrance? We might remember that in Sidonius's poem of Burgus we are told that a stone was set at the entrance boasting the founders' names. The walls and gates of the inscription conjure images of a walled town. Now the inscription stands along

the path of easiest access through the hills, only some half kilometre from where a natural line of enclosure starts and runs approximately twelve or thirteen kilometres, enclosing a natural plateau of roughly 800 ha. It is doubtful that a dressed stone wall enclosed the entire circuit, perhaps an earthen bank or even nothing but nature marked most of the boundary. A short stretch of walling and a gate entrance, however, could be plausibly postulated at both the south-western and north-eastern entrances to this natural plateau, whence stray finds commensurate with a fifth-century date have surfaced, and still fulfill the description of Theopolis given by the inscription.

Bibliography: Châtillon 1943, Marrou 1954.

The immediate image conjured by the above texts is that of classical Roman villas. Hypocausts and hot baths, *triclinia* and towers, gilt ceilings and cement, lead pipes and painted walls, colonnades, and even a library. In another letter of Sidonius we even hear of a hydraulic organ (*Epist.* 1.2.9).

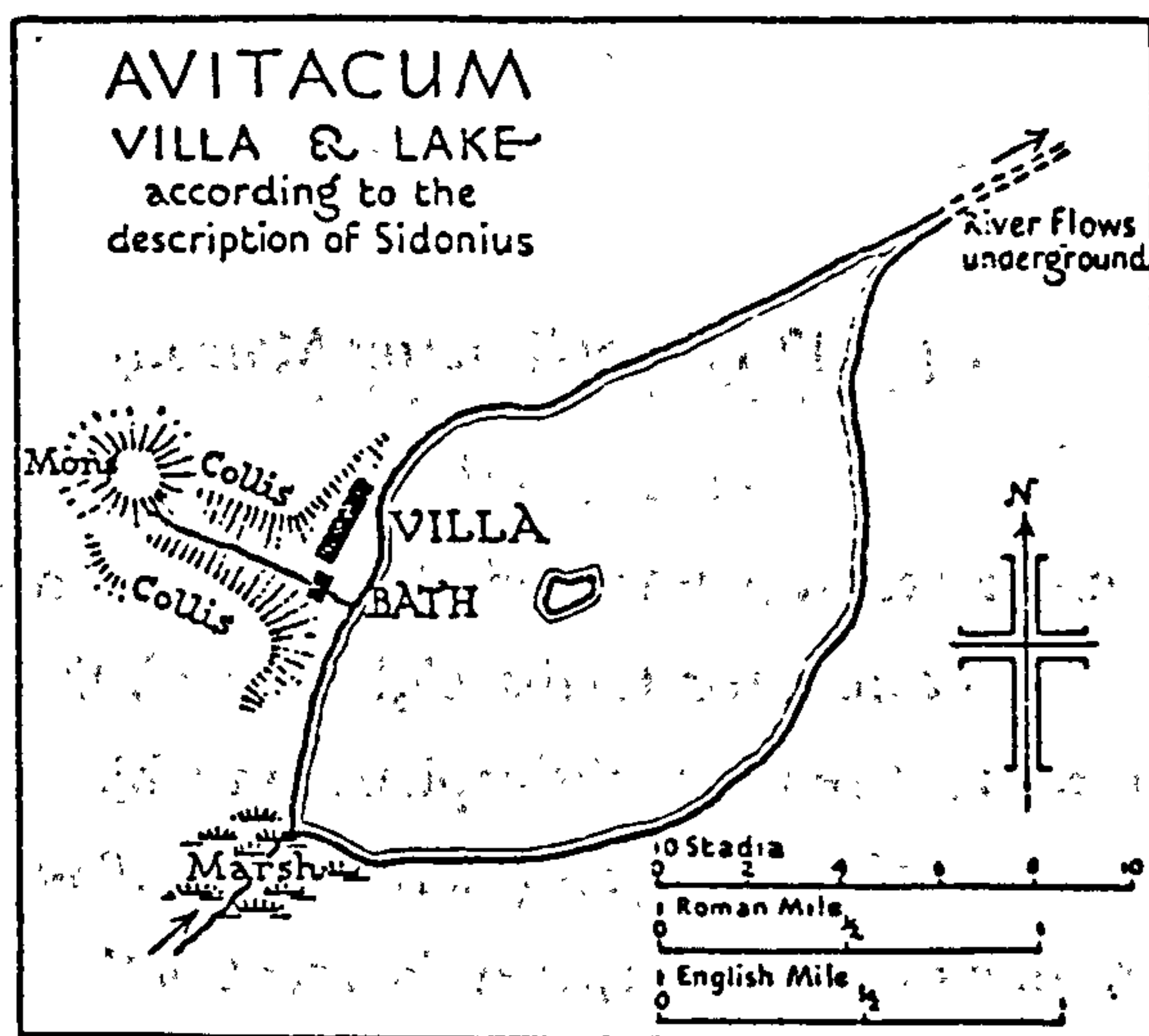


Fig. 3.13 Probable location of Avitacum, according to the description of Sidonius Apollinaris, thought to be under the village of Aydat (after Stevens).

The topographic siting is similarly Roman. Overlooking a river is a common feature. Although we cannot pinpoint Avitacum exactly, it clearly overlooks Lac d'Aydat near Clermont (fig. 3.13). Columella (*De re rustica* 1.2.3) suggested that villas be oriented to the rising sun, and the archaeological evidence shows that Gallo-Roman villas followed that advice (Ferdière 1988, 95). Palladius (*Opus agriculturae* 1.7), closer in time and space to Sidonius, gave the advice of building

to face the east or south in cold regions, no doubt to better enjoy the sun and avoid the prevailing winds. We should note that Avitacum, Burgus, and Paulinus of Pella's villa all appear to have followed this common Roman practice, being oriented away from the north or having rooms appropriate to different seasons.

The description of Burgus is well enough known, although the investigation of A. Nicolai (1929) seems less so, indeed, it fails to appear in almost every bibliography. Percival notes that none of the forementioned villas has yet been satisfactorily identified on the ground, but that a kilometre from Bourg-sur-Gironde a villa has been located. Percival cautions that it has never been explained how the name came to be transferred to the village if this villa was indeed Burgus. Again, Salin footnotes the report, but no mention is made in the text of the possible location of Burgus, while both Fournier and Rouche mention Sidonius' poem, but ignore Nicolai (Salin 1950-59; Fournier 1978; Rouche 1979). Higounet (1963, 209) alone seems to accept Nicolai's assertions.

Nicolai rightly dismisses some of Sidonius' exaggeration. The limestone cliffs along the north bank of the Dordogne and Garonne after the confluence, nowhere exceed a height of 50 metres, no 'mountain' will be found 'piercing the sky'. Sidonius' siting of Burgus at the confluence but nearer one river than the other must put it somewhere on the right bank of the Dordogne, just east of the bec d'Ambes, where it can be no accident that Bourg-sur-Gironde is situated. At Gogues Nicolai investigated what clearly was the remains of a villa. A hypocaust was found under part of a large area paved with bricks (*briques*), which was held up by square columns of bricks sitting on a thick layer of mortar. All this unfortunately had been ripped up by the proprietor at the time, looking for treasure. Cement-lined channels were found and fragments of conduit tiles abounded. Nicolai was in no doubt that this villa was Burgus when he found the walls and towers. The foundations of the first square tower apparently stood three or four metres above ground level. It projected from the wall which formed the corner of the facade facing the Dordogne. In all some three or four towers were found along this facade. The walls were faced with courses of regular *petit appareil* with intermediary courses of brick. Unfortunately, no plan is given, nor any location of the walls any more exact than Les Gogues. Today the area is completely covered with private dwellings and vineyards, so that if any remains are still visible, they are rather inaccessible. We are informed that the plough never ceases to bring debris to the surface, although for dating purposes, we are no further enlightened than that domestic pottery, red, black, grey, and white was abundant, found alongside nails and fragments of marble.

Attempts to draw firm conclusions from the foregoing evidence are fraught

with danger. The appearance of baths in all the literary examples given above should not, for example, be taken to imply they were common features. Perhaps their very rarity made them worthy of praise, which was the purpose of all the documented passages quoted above. What can be seen is the marked continuity of Roman architecture, even if these villas were exceptionally fine by contemporary standards: lead pipes, running and heated water, hypocausts, columns, cement and marble, baths, sleeping quarters, dining rooms (*triclinium* and dining couches – was the Roman style of reclining while eating still practiced?), weaving-rooms, and separate granary buildings all speak of *romanitas*.

At Burgus, we are told, a stone was set up in the grounds with the founders' names engraved upon it. At Theopolis we have just such a stone.

Of the two postulated villa developments, there is no positive evidence of nucleation, but clear expression of fortification in the case of Burgus. Taken together, I believe *contra* Wightman (1970, 169), that Sidonius and Nicolai imply that the walls and towers of Burgus, like Pfalzel (Wightman also suggested the parallel), did not encircle the villa like a mediaeval curtain wall, but presented the external face of the villa. This seems certain from the implications Sidonius makes that rooms in the towers were connected directly with other rooms in the villa.

The Italian villa of Le Mura di S. Stefano near Anguillara Sabazia, 26 km. north-west of Rome, provides an interesting comparison (fig. 3.14). A rectangular building, 17 x 21 m. and still standing to a height of 18 m., built of brick-faced concrete, constructed in the mid-second century, has been interpreted as forming part of a villa complex (Lyttelton 1980), although whether as a wing or central domestic range is disputed (as indeed, a minority opinion disputes its interpretation as a villa). Recent excavations by David Whitehouse (1982) have thrown light on its post-Roman history. After ca. 400, the edifice was made more defensive by the digging of a ditch and it is suggested that this was the date at which the ground floor windows were blocked. Perhaps Burgus too had no ground level windows or doors, but for the main entrance.

Elements of the architecture at Pfalzel, and perhaps thus at Burgus, are to be found in earlier villas. The central courtyard is a common feature, and the *galerie de façade* villas occasionally presented a gallery running between two towers, as at La Chapelle-Vaupelteigne and vividly depicted in a Trier fresco (fig. 3.15). Simply replacing the gallery facade with the external villa wall would have produced a facade similar to that of Pfalzel. Similarly, the winged corridor villas, in their less developed stages presented such a facade, with projecting corner

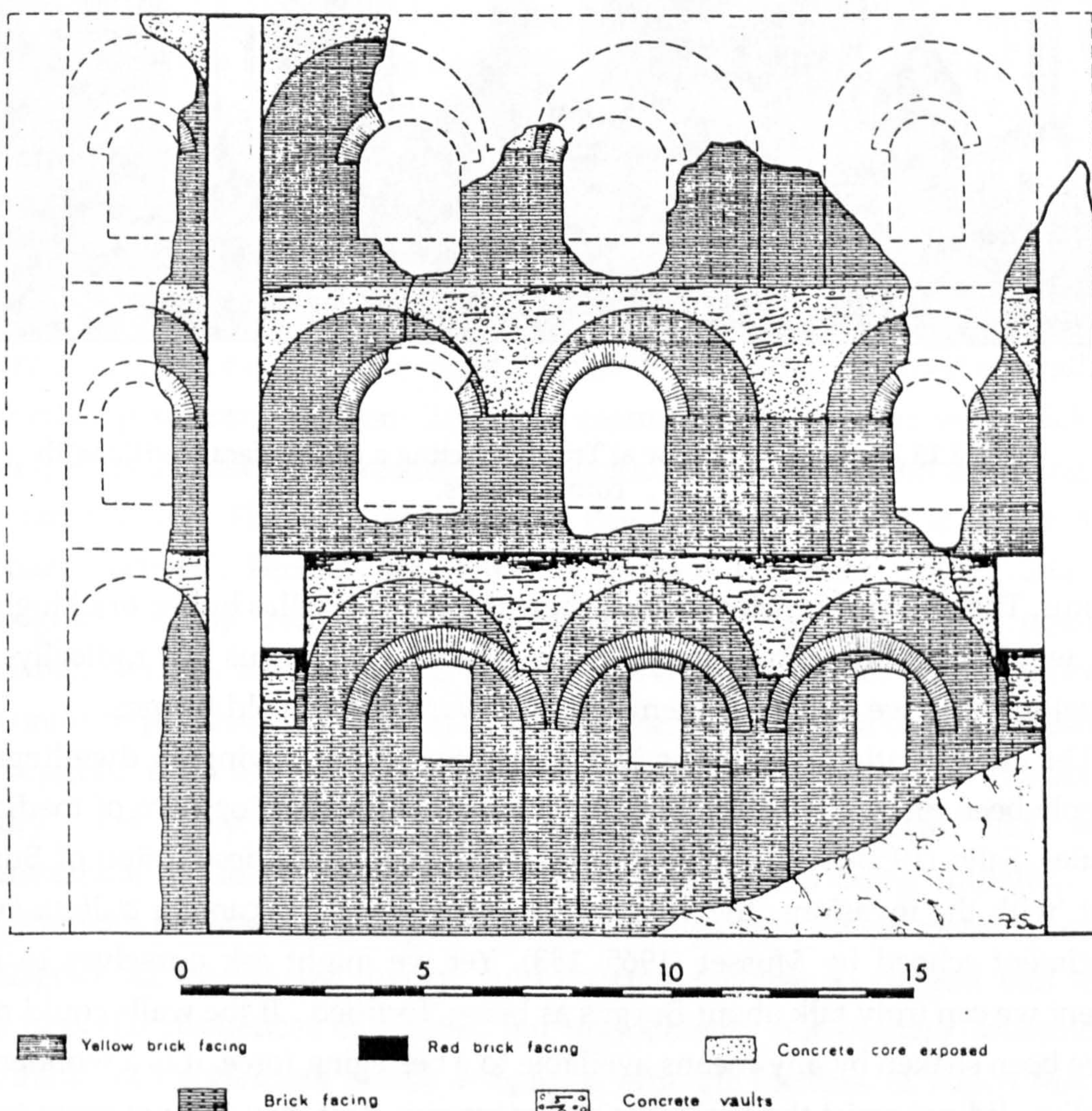
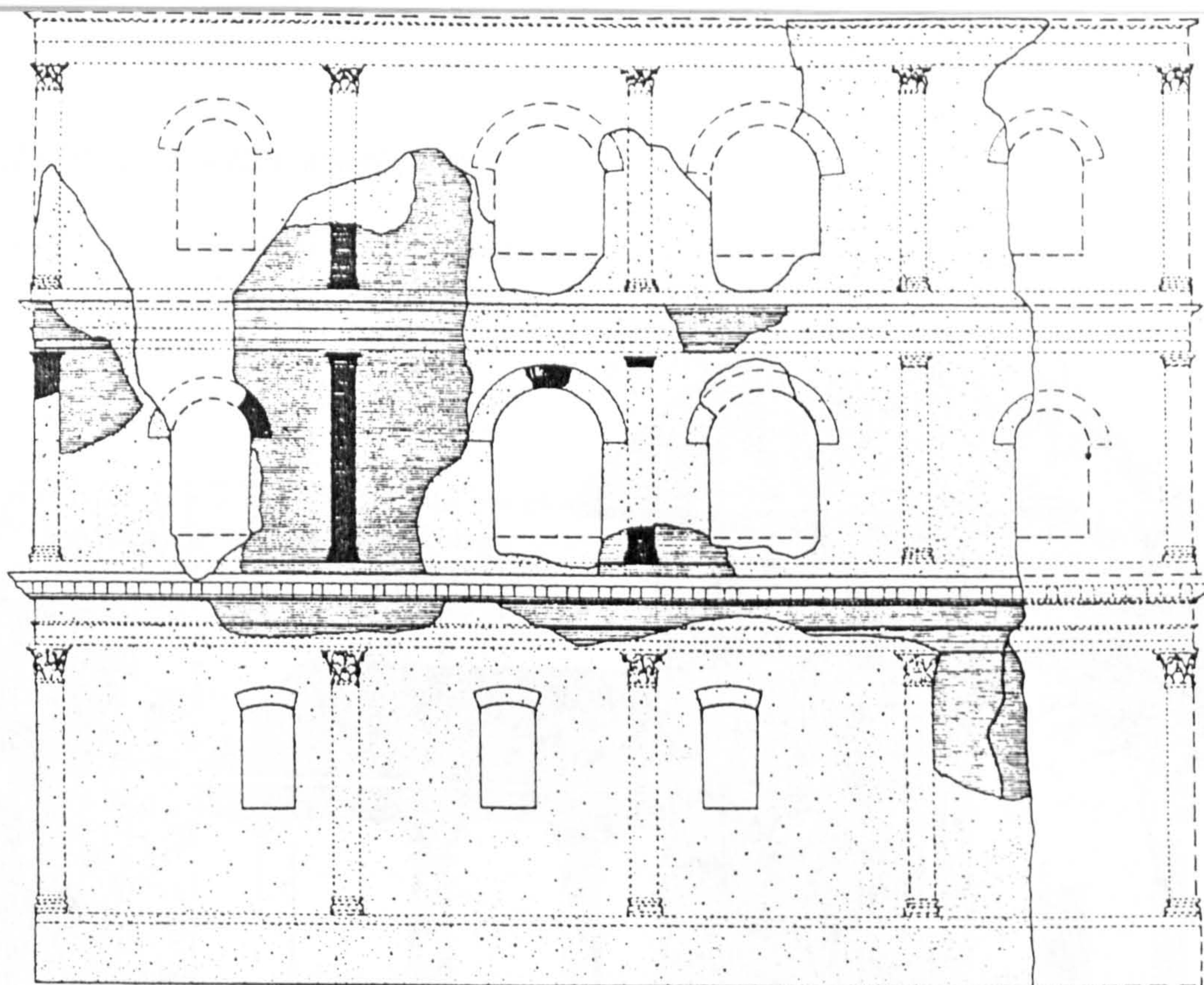


Fig. 3.14 Le Mura di San Stefano villa: elevations of the west wall exterior and interior. Rectangular tower forming part of a Roman villa near Rome. Sometime in the late empire the ground-floor windows were bricked up (after Lyttleton).

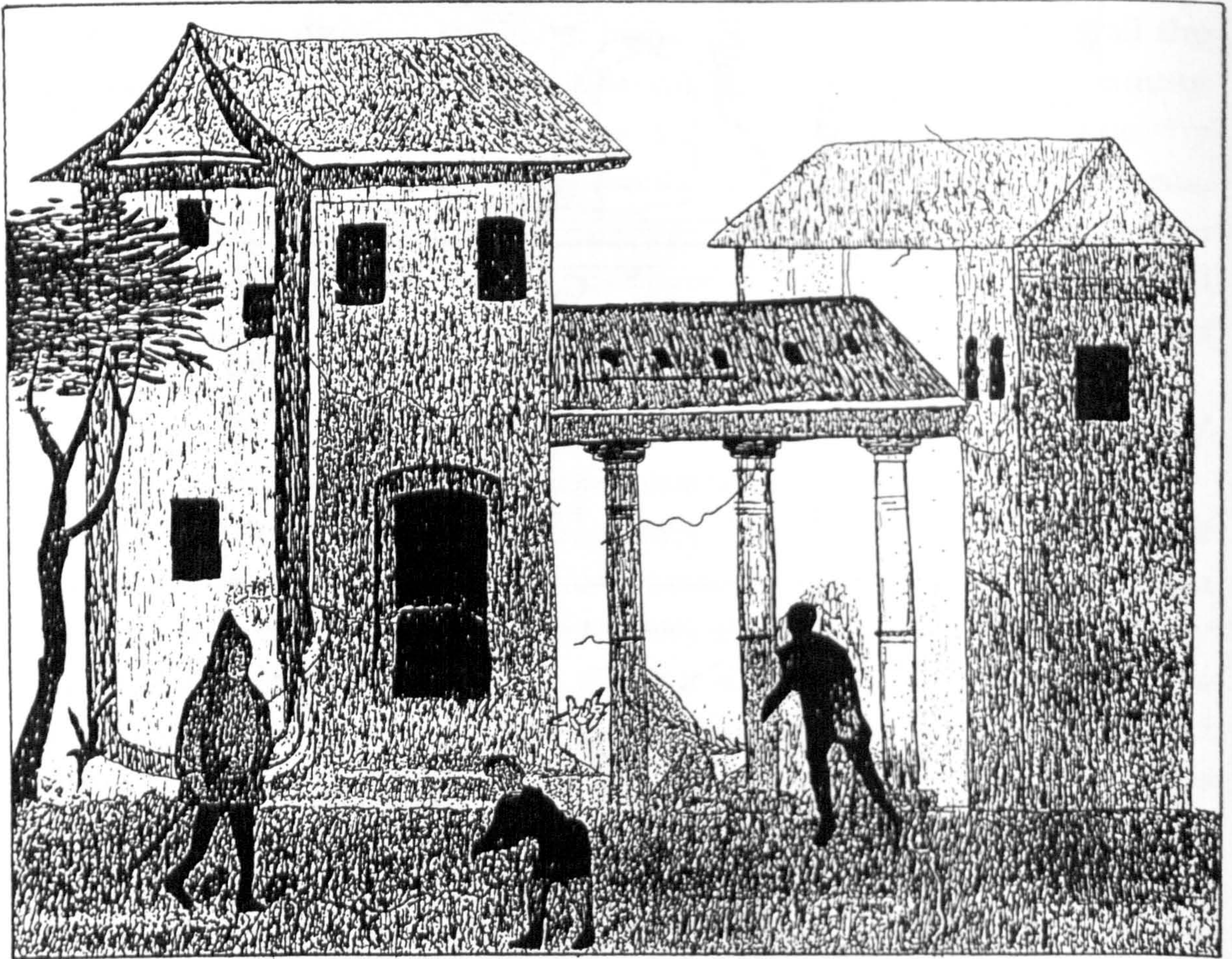


Fig. 3.15 Roman fresco, now at Trier, depicting a gallery facade villa with corner towers.

rooms. The tower of Le Mura joined these donjon-like villas by the bricking up of its windows, and rather than seeing Pfalzel or Burgus as radically new developments, we might see them simply as variations on old themes.

The concentration on Burgus in the literature on Merovingian dwellings has openly been the result of attempts to foreshadow the development of mediaeval castles. Salin (1950-59, 416) claimed, following Sidonius's description of Burgus, that 'with the invasions, the villa of the *grand seigneur* became a *château-fort*', a sentiment echoed by Musset (1965, 183). Yet we might ask ourselves to what extent we can truly talk about Burgus as being 'fortified'. If the walls could never have been shaken by any means available to a besieging force, it is a wonder that Pontius did not resist the Visigoths and moreover, that they did not seem to fear his resistance. The architecture was perhaps designed more with the intention of display in mind. As Sidonius (*Epist.* 2.2) remarked of the gilded ceiling, 'the rich

prosperity of the house, brooking no secrecy, reveals its wealth when thus it hides its roof.' The lay-out and architecture of Consentius's villa was, in the words of Sidonius, conspicuous. A quote from Grenier (1934, 464) sums up my line of argument:

I have looked in vain in Gaul for examples of fortified villas such as one finds in Africa. The villas, doubtless, may give the appearance of a fortress by elevating the corner towers at the ends of their facades composed of an open gallery, for example. These are but the games of architects.

The corner towers, according to Grenier, gave only the illusion of fortification as part of an architectural game. But what was the game being played?

One possible source of inspiration for this architecture comes from Roman military building. A comparison of late Roman *burgi* and Pfalzel is occasional made. It is interesting that Burgus, probably similar to Pfalzel, had a name which not only implied a defensive nature, but was identical to the name applied (at least it is by archaeologists today) to the smallest of military defensive sites; watchtowers. These vary in size and design like all late Roman walled sites, so that large *burgi* blend into small forts. The similarity of the plans of forts from Arabia and Raetia (fig. 3.16) to that of Pfalzel is striking, but is it surprising?

When offering examples of fortified villas, Percival notes the *castella* villas of Tripolitania (the solidly built olive farms mentioned above by Grenier, ignored for the purpose of this study) and the fortified villas of Panonnia. These villas present a number of common features (fig. 3.17): large areas enclosed by walls of rectangular plan, ranging from 2.3 to 2.6 metres wide and thus very thick by contemporary standards, furnished with towers, usually round and adjoining the wall tangentially. The area enclosed at Pandorf was 'almost as large as a legionary fortress', Fenekpuszta with 44 towers measured 392 by 348 m., Gamzigrad 300 by 230 m., Helenypuszta, Sagar (ancient name possibly Tricciana), Kornye, and Mursella, and A. Mocsy (1974, 305-6) doubts whether such uniform planning and lay-out could have been carried out on private estates and suggests that 'fortification of private villas towards the end of the fourth century was only exceptionally permitted.' Some concrete and more circumstantial evidence points to all these constructions being imperial. The characteristics of these sites, huge palatial dwellings with lavish mosaics, and at Gamzigrad 'all imaginable luxury', and enormous *horrea* all suggest that these were imperial residences or estates, but undoubtedly not military settlements. One thinks of Diocletian's palace at Split, a veritable fortress, of which Mackay (1975, 208) remarks, 'This marriage of villa and *castrum* was no isolated phenomenon.'

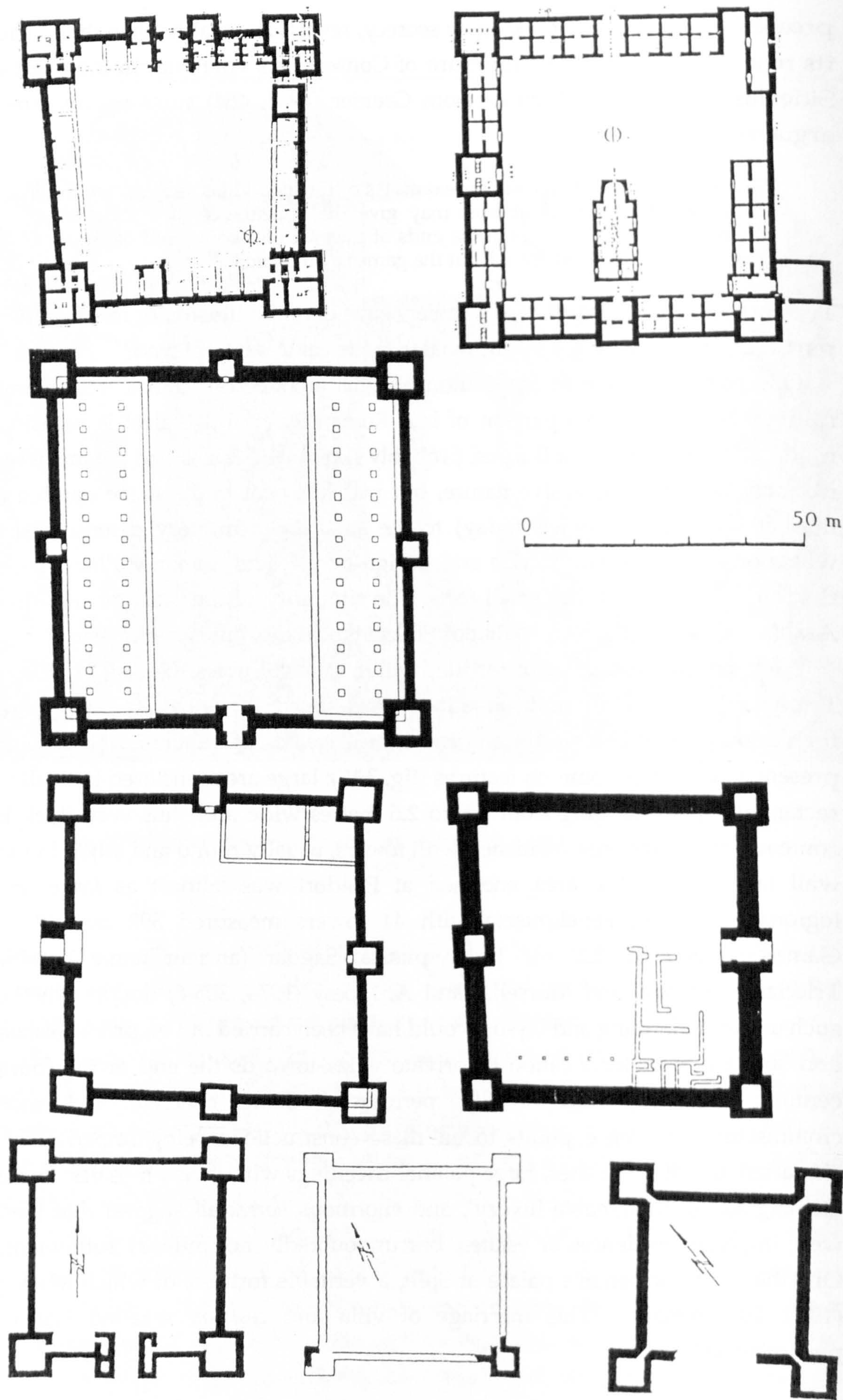


Fig. 3.16 Roman forts: Qasr Bshir (A) and Deir el-Kahf (B) from Arabia and Innsbruck-Wilten (C), Irgenhausen (D), Schaan (E), Gornea (F), Ravna (G), and Dierna (H) from Raetia (after Lander 1984).

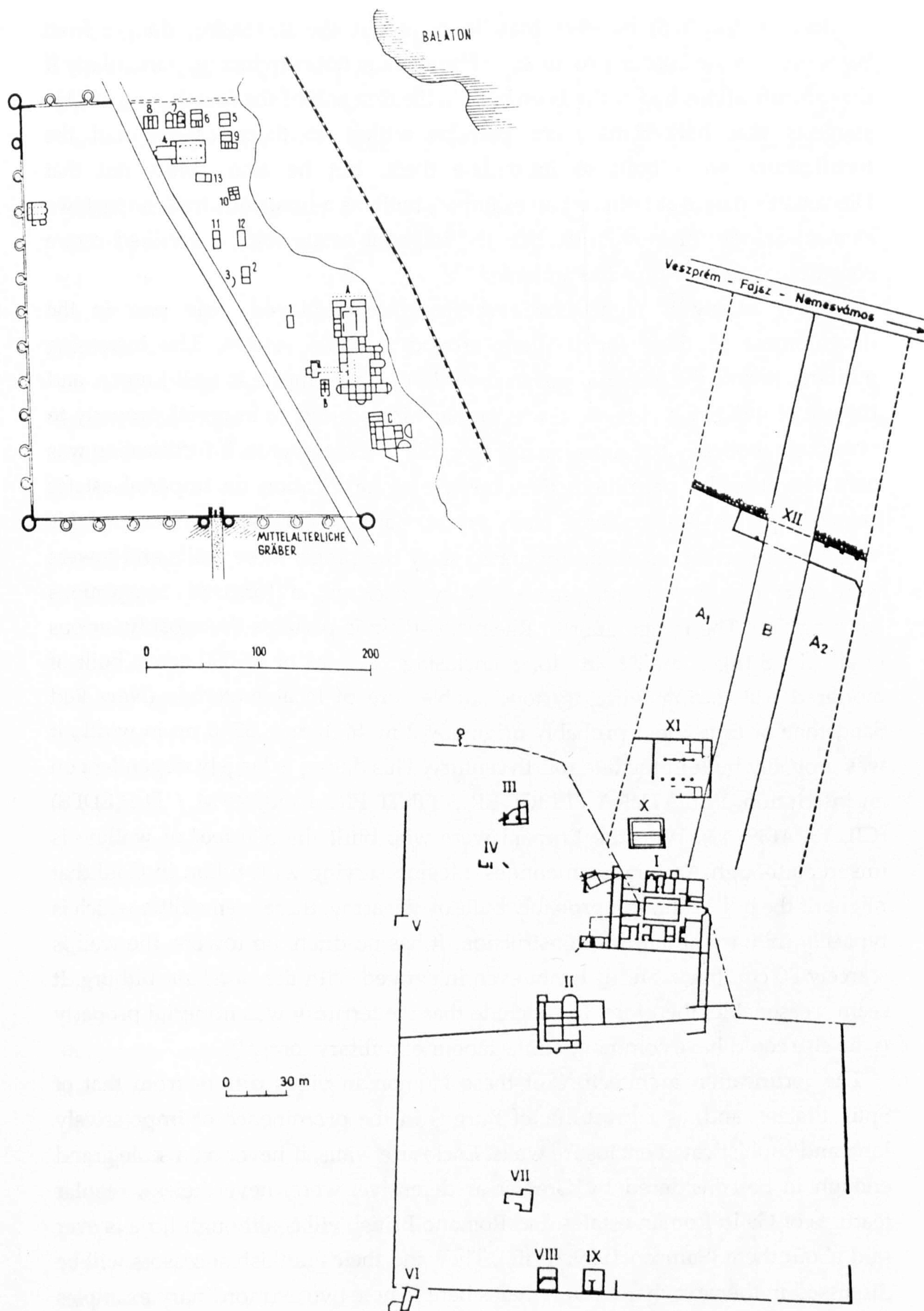


Fig. 3.17 Two villas of Pannonia: Keszthely-Fenekpuszta, and Nemesvámos-Balácapuszt, Hungary.

Mocsy (1974, 306) believes that 'to point out the increasing danger from barbarians in the border provinces of Pannonia is not convincing, particularly if these fortifications had really been built in the first half of the fourth century.' He suggests that barbarians were perhaps settled at these estates and the fortifications were built to intimidate them, but he also points out that Diocletian's palace at Split 'set an example', built 'as a luxurious fort', so that 'we cannot exclude the possibility that the imperial estate with a fortified centre constitutes an estate-type in Pannonia.'

Clearly Mocsy is right that barbarian threats played little part in the development of these fortifications around imperial estates. The increasing militarisation of the imperial government in the late Empire is well-known, and the use of military architecture was perhaps introduced to imperial property to project symbolically the supreme military role of the emperor. If fortification was only exceptionally permitted, then the use of fortification on imperial estates helped separate the emperor from private land-owners and underlined his singularity. Further, as militarily unnecessary trappings, these walls and towers were part of the 'all imaginable luxury' package; a form of conspicuous consumption. The Langmauer in Rheinland-Pfalz is perhaps the most luxurious of all (fig. 3.18). Some 72 km. long, enclosing 220 km² or 55,000 acres, built of mortared wall facing with drystone rubble core of local materials (New Red Sandstone or limestone) probably originally 2 m. high and .65-.8 m. in width, it was probably built in the late fourth century. This dating is largely dependent on an inscription, PEDATVRA FELICITER / FINIT PRIMANORVM / D P(EDES) (CIL 13, 4139/40). Who the Primani were who built the 500 feet of walling is unsure, although Ammianus mentions a legion serving with Julian in Gaul that might fit the bill. Although probably built by the army, there seems little which is typically military about this construction. It has no ditch, no towers, the wall is scarcely 80 cm. thick, and it is not even integrated with the *castellum* Bitburg. It seems reasonable, therefore, to conclude that the territory was imperial property (who else could have commanded the labour of military forces?).

The fortification architecture of these Pannonian villas differs from that of Split, Pfalz, and, as I presume, of Burgus in the prominence of impressively long and sophisticated enclosure walls. Enclosure walls, if never on a scale grand enough to be considered by Grenier as defensive, were, nevertheless, regular features of Gallo-Roman estates and Romano-British villas although little is ever said about them (Samson 1989; forth.). They and their Frankish successors will be discussed in the next chapter. It remains here to note two extraordinary examples from the fifth century.

A possible villa of the fifth century which, like the imperial villas of Pannonia and the possibly imperial estate of Langmauer, had possibly chosen to raise an enclosure wall to the status of fortification, is the elusive *locus* named Theopolis, lost in the upper reaches of the Jabron, a tributary of the river Durance (fig. 3.12).

Concluding Remarks

The appearance and architecture of fifth-century villas is hard to ascertain, but the evidence available does not suggest they were fortified. This will also be found to be true of Merovingian villas in the next chapter. While I suggest that fortification, as one of Percival's two postulated developments of villas in the fifth and subsequent centuries, may be discarded, the evidence for nucleation is too sparse to evaluate. Literary evidence does, to my mind, suggest the existence of villas in the late fifth century little different from those a century earlier, but they have yet to be found by the spade. Our understanding of fifth-century villas will continue to rest on the theories we present to explain their archaeological invisibility.

Surface collections, particularly of pottery, are almost useless for the purpose of establishing the date of abandonment or continuity from the fifth century onwards, given the overall decline in the amount used and the almost total absence of highly diagnostic and well-dated pottery. If building in timber and drystone became more common in the fifth century, as we suspect, the ability to spot villas from the air is markedly lessened, and only good and extensive excavation can hope to uncover the necessary information. The nature of the archaeological record guarantees that post-Roman villas will be less well represented than their Roman counterparts.

A further explanation for the invisibility of post-Roman villas is that their very success as settlements has left them buried under centuries of subsequent occupation. This explanation has been supported by attempts to show *-acum* villages as probable descendants of Roman villas. They are much more likely than other named villages to yield archaeological evidence of Roman villas, and the known distribution of Roman villas often makes little sense unless we postulate their former existence under these villages.

Some sites exist, such as Tholey, that make it almost certain that the structure of the Roman villa was still standing in the seventh and eighth centuries, and this despite the almost total absence of positive archaeological evidence for continued occupation or use. The extreme care needed to date the final destruction, collapse, or abandonment has already been discussed in the previous chapter in relation to major Roman municipal buildings and town walls. Among Roman villas too it

would seem that insufficient thought has been given to the problems of dating final abandonment. Many more fourth-century villas may have survived into the fifth, sixth, and seventh century than has generally been believed.

Finally, the frequency with which we meet Roman villas under churches has led to the hypothesis that the churches originated as *Eigenkirchen* on secular or ecclesiastical property. It has been argued in this chapter that such churches were by no means common in the fifth century and that this phenomenon thus speaks even louder for continuity, for the churches established over former Roman villas that were to become important for the whole parish were often not founded until the sixth, seventh, eighth, or even ninth century. Such continuity was only possible given villas' continued prominence as centres of agricultural lordship.

Chapter Four

Merovingian Villas, Palaces, and Estates

Royal Urban Palaces

The theme of continuity, particularly through the royal acquisition of imperial property and adoption of imperial practices is in many respects no more than received wisdom for the sixth century. As early as 1873 C. Martin-Marville suggested the continuity of Roman architecture in Merovingian villas. In his fortunately uninfluential paper Martin-Marville suggested that royal Merovingian and Carolingian villas took Roman *fora* as their models, producing a characteristic rectangular courtyard. Most commonly, Martin-Marville (1873, 370) suggested, there were two courtyards: the higher for the use of the 'prince', the lower for the 'leudes'. Most of the sites of his gazetteer yield only documentary evidence of being royal from the tenth to thirteenth centuries and the plans of ditches and ramparts leave the impression that they date to a similar period. The upper and lower 'courtyards' of Marville's 'villas' were of course no more than the remains of motte and bailey constructions. The assumptions of archaeologists have been heavily biased by those of historians, and from at least the early nineteenth century when Augustin Thierry wrote, it has been accepted that the Merovingian kings simply took over imperial estates, imperial taxation, and all other forms of imperial revenue. This view of continuity, we will see, has assumed a life of its own among historical works, although based largely on assumptions, albeit reasonable assumptions. How much so, is revealed in a statement by James Thompson (1935, 1): 'We know the name and location of only one among what must have been hundreds, mayhap thousands, of the former imperial domains in Gaul.' But one domain alone is not proof of the continuity between the imperial and the Merovingian fisc.

Whether we are entitled to be as optimistic as Thompson is another question. Todd (1988, 15) says 'the question of Imperial estates [is] much discussed and often in erroneous terms. There is surprisingly little firm evidence for these in the western provinces and their extent may have been exaggerated.' There was one in

Gaul by Thompson's reckoning. The rest of the western empire is little better documented (Crawford 1976). Continuity between royal fisc and imperial estates ought to be approached more cautiously when almost nothing is known of the former.

To Thompson's one, we may add Pfalzel, discussed at some length in chapter three. It was definitely in royal possession and circumstantial evidence points to it having been imperial. But if Pfalzel reveals continuity of ownership, it does not appear to reveal continuity of occupation, for Fortunatus's poem seems to imply that it had fallen into ruins in the sixth century. Where evidence arguably exists of royal Frankish occupation on former imperial Roman property is in the towns.

ROYAL MEROVINGIAN URBAN PALACES

| | |
|---|----------------|
| 1. Chalon-sur-Saône <i>palatium</i> with <i>oratorium</i> | 11. Autun |
| 2. Köln <i>aula regia</i> | 12. Bordeaux |
| 3. Metz <i>palatium</i> | 13. Cambrai |
| 4. Orleans <i>sedes</i> | 14. Clermont |
| 5. Paris <i>sedes</i> | 15. Koblenz |
| 6. Reims <i>sedes/domum regia</i> | 16. Lyon |
| 7. Rouen <i>domus regalis</i> | 17. Macon |
| 8. Soissons <i>palatium</i> | 18. Mainz |
| 9. Trier <i>palatium</i> | 19. Meaux |
| 10. Vienne <i>palatium</i> | 20. Nevers |
| | 21. Poitiers |
| | 22. Rodez? |
| | 23. Strasburg? |
| | 24. Tournai |
| | 25. Tours |
| | 26. Verdun |
| | 27. Zülrich |

Table 4.1 is derived principally from three works: that of Thompson (1935), Bergengruen (1958), and Weidemann (1982). Bergengruen's work is derived almost entirely from charter evidence, many of which cannot be trusted. A number of sites listed in Thompson's work that he included on the testimony of Gregory of Tours have been rejected here, both on the grounds of Weidemann's interpretations of Gregory's work and of my own reading of the texts.

The list is divided into two parts, those sites at which a royal palace is definitely recorded and those at which palaces were probable, given the information of extended royal visits to these towns. The division is therefore not one of substance, but one of certainty.

It is permissible to infer that any long sojourns by Merovingian kings within cities were indeed on their private property and not as guests of the resident bishops, for such was a practice which only grew in importance following the reign of Charlemagne, as Brühl (1968) has shown. Laconic references by Gregory (e.g. HF 6.31) to the burden placed on the population, such as in Paris, suggests

perhaps a taxation. The most telling anecdote, however, concerns King Guntram's visit to Orleans in 585. Gregory of Tours entertained Guntram shortly at Gregory's own dwelling (*ad metatum nostram/in mansionem meam*) but Guntram stayed in his own residence and asked the assembled bishops to visit him there (*in domo mea*) (HF 8.2). Furthermore, we are told that Guntram was entertained in the *domus* of locals which James (1982, 57) suggests could hardly have been hovels and were perhaps the town-houses of great men.

Because of the paucity of information, itineraries for Merovingian kings cannot be produced. However, an overview of royal residences can be, based on the available evidence and has been most authoritatively expressed by Ewig (1963) and Brühl (1968). A traditional view had been that Merovingians maintained a Roman practice and stayed almost exclusively in cities while the Carolingians shunned urban centres for their rural estates. Brühl (1968, 13) stresses that the distinction was much less rigid, that the Merovingians frequented their rural villas very often, and that Carolingians were no strangers to cities. Indeed, Merovingian kings may have spent a majority of their time on their rural estates.

'It is certainly not to be explained as a simple accident of survival when the preponderant majority of the near one hundred genuine Merovingian diplomas were issued from villas not cities.' Perhaps as few as three genuine charters and two capitularies appear to emanate from *civitates* (Brühl 1968, 12).

The charter evidence thus emphasises the rural estates, but the other documentary evidence thrusts urban centres to the forefront. This is particularly apparent in relation to the idea of a capital, which Ewig (1963) discusses. The earliest Frankish kings are found residing at Tournai, Cambrai, Tongres, Köln, and elsewhere. Clovis chose Soissons, and later Paris as his main centre. With Clovis's death the new *sedes* included Orleans, Paris, Soissons, and Reims, Orleans being later replaced by Chalon-sur-Saône, Reims by Metz. Before Clovis's expansion, Burgundian and Visigothic *sedes* existed in Geneva, Lyon, Toulouse, Bordeaux, and Narbonne. Brühl (1977, 423) suggests that one reason for the preponderance of charters from rural villas is that they post-date 625 and that the earliest Merovingians in fact resided more frequently in their urban palaces than we can know. A reason for this, he argues, is that the Merovingians ruled as *magistri militum* rather than as conquerors (Brühl 1977, 423-4):

What could be more natural than this authority being exercised at the place which was familiar to the Gallo-Roman population and was already, from ancient times, the seat of Roman magistrates, namely from the *praetorium*?

In fact there is not a great deal of evidence for Roman magistrates in *praetoria*, which are archaeologically rare in the western empire; that the population was

familiar with authority being exercised there is something we could only suppose. In the *Codex Iustinianus* (1.40.14) we do have a proclamation of the eastern emperor, Leo I, dating perhaps to 471, which informed provincial officials to use (presumably imperial) palaces as their bases. Where *palatia* and *praetoria* existed, the governor (*praeses*) was to use the *palatia* for his residence and the *praetoria* as magazines and as 'granaries' for the public treasury. Rudolf Egger (1966) offers one of the few discussions of this take over of the *praetoria* and *palatia* by high-ranking officials in the late empire.

What these urban palaces of Merovingian kings were like is difficult to say, using the laconic documentary sources. Of ducal and comital urban residences, we know as good as nothing. However, of episcopal palaces we are better informed. Of the *domus ecclesiae*, we have a vivid summary of its functions, placed by Edward James (1982, 54) in its architectural setting:

It was a law-court, boarding-school, seminary, hotel and citizen's advice bureau all in the same building . . . The urban clergy often lived in the same house, ate with the bishop, and frequently slept in the same room. In a separate establishment might well have lived the bishop's wife, the *episcopa* or *episcopissa*.

Although the *episcopum* or *domus ecclesiae* is frequently mentioned by Gregory, there are only a few anecdotes which give good architectural details. Priscus, the bishop of Lyon, ordered a new storey to be added to the house at the beginning of his episcopate (*Iusserat enim in primordio episcopatus sui aedificium domus ecclesiasticae exaltari*). A mad deacon climbed onto the roof and managed to take off tiles before a beam collapsed and he fell to the ground, crushed to death (*hic ascendens super tectum domus illius, cum detegere coepisset . . .*)(HF 4.36). Coincidentally another useful passage similarly refers to the roof of the episcopal dwelling. Fleeing Childebert's men, Berthefried came to Verdun and took refuge in the oratory of the church-house (*in oratorio qui in domo aeclesiastica erat*), thinking he would be all the more safe as Bishop Ageric had his residence in the same house (*in hac domo resederet*). Childebert's men climbed the roof of the oratory and removed its tiles and material (*ab ipsis tegulis ac materiis quibus oratorium opertum erat*) in order to gain entrance to kill Berthefried (HF 9.12). In neither case is it explicit that the *domus ecclesiae* was built of stone, although the assumption is probably valid. Heightening a wooden building by the addition of an extra storey is much more difficult than heightening a building with stone walls, so the *domus ecclesiae* of Lyon was probably stone built. The *tegulis* and *detegere* of the two anecdotes similarly point to a classical building tradition.

Of royal palaces Gregory says even less. In passing, it will be seen from table 4.1, that *palatium* appears in four cities, *aula regia* and *domus regalis* each once. Of

these, the only really informative passage is that relating to Metz (HF 8.36):

As the king, who was in residence at his palace in Metz (*aula regia*), was watching some wild beast which was being harried from all sides by a pack of hounds, Magnovald was summoned to his presence. Without having been told the reason for the summons, Magnovald came and stood watching the animal, laughing loudly with the others. A man who had been told what he was to do came up to the group and, when he saw that Magnovald was intent upon the sport, pulled out his axe and split his skull. Magnovald fell to the ground dead and was thrown out of the window of the house.

This passage informs us that there was an open area around the palace in which the spectacle could take place and almost certainly that it was enclosed to prevent the wild beast and dogs from escaping into the town. Otherwise it might be added that the description of Magnovald being thrown out of the window may imply that they had been watching the spectacle from an above-ground storey. But we cannot even say that this is suggestive of a continuation of a Roman town hall.

It is arguably on the urban residences that the strongest assumptions are based for the continuity of Merovingian estates from Roman times and the fifth century. This chapter began by quoting Thompson, who noted that there was only one demesne that was demonstrably an example of imperial Roman property held by Frankish kings, that of Tournai. By a stroke of fate, Tournai found itself recorded in the *Notitia Dignitatum* because of the military workshops founded there. That it became a royal fisc is supposed by Pirenne (1925), although its first appearance as such was not recorded until the beginning of the ninth century. Pirenne suggested that it remained in Merovingian hands long after most of their others were lost, because of the sentimentality the site may have held – Clovis had originated as *rex* of Tournai.

Although Tournai is the only Roman imperial demesne in Gaul that we know, it is often assumed that every urban centre had its administrative headquarters which was taken over by Merovingian royalty. Brühl (1968, 10) believes that the Frankish *palatia* of the sixth and seventh centuries were in truth '*die alten römischen regiae oder praetoria*', which the kings assumed with the fisc as 'legal' inheritors. This belief can really be neither proved nor disproved, much as it is generally accepted. Several examples may help to illustrate Brühl's methodology and the slenderness of the supporting evidence.

Paris. According to the *Notitia Dignitatum* the *praefectus classis Anderetianorum* was stationed in Paris. In 360 the emperor Julian and in 365 the emperor Valentinian I resided in Paris. After Gratian's army was destroyed here in 383,

Paris does not reappear in documentary sources until after the battle of Soissons in 486 when Clovis established his seat at Paris (*Parisius venit ibique cathedram regni constituit*) (HF 2.38). Where the *praetorium* was located, in which Julian and Valentinian had undoubtedly resided, is today conjectural. Outside the city walls seems unlikely, so therefore the theory runs, it must have been located on the walled *île de la cité*, undoubtedly on the site of the *palais de justice*. Clovis, it is assumed, would have resided on the same spot one hundred years later.

Soissons. In the fourth century Soissons held a garrison and a weapons factory. Although no imperial visits are recorded, the *magister militum*, Aegidius (d. 464), and his son Syagrius resided here. It became one *sedes* following Merovingian divisions of the *regnum*, and Chlothar I, Chilperic I, Theudebert II, and Dagobert I all resided in Soissons on occasion, presumably on the same spot as had Aegidius. Again the *praetorium* can be assumed to lie within the late Roman walls and Brühl suggests the north-east corner where the '*tour des comtes*' was destroyed in the eleventh century, while the present-day site of the *palais de justice* is not suggested.

Additional to Brühl's arguments we could add those of Kaiser (1973, 195 ff). Why should Aegidius and his son Syagrius have chosen Soissons as their main centre? 'It is difficult to understand the decision of Soissons [as their *sedes*] if not on the military/economic basis of imperial fiscal property . . .' Within the boundaries of the *civitas Suessones* lay the very important Merovingian and Carolingian royal villas of Quierzy, Montmacq, Choisy-au-Bac, Compiègne, Verberie, and Berny-Rivière. If these had all been part of the imperial fisc, the choice of Soissons as a *sedes* by Aegidius, Syagrius, and later Merovingians would become instantly understandable.

Chalon-sur-Saône. According to the *Notitia* the *praefectus classis Araricae* was stationed in Chalon. Although imperial visits were rare, Chalon became an important royal city for the Burgundians from about 500 and for those Merovingians to whom the Burgundian share of the *regnum* fell. No royal charters prove a royal Merovingian residence, but Fredegar (4.90) mentions a *palacium* which the patrician Willebadus refused to enter in 642. A column capital has been suggested as originating from this palace (Brühl 1975, 135). The site suggested is the south-west corner of the Roman wall, where later Burgundian dukes had a castle, which was, supposedly, the fourth century Roman *praetorium* because of its characteristic position: 'on the basis of this

characteristic position, it can be accepted with certainty that this was the site of the Roman *praetorium* in the fourth century' (Brühl 1975, 135).

Brühl's method, which is left in silence in his book (1975) and only revealed in a later general survey (1977), is first to locate the late Roman town walls and probably quite rightly to assume that the *praetoria* – by which he understands (1968, 10) an imperial residence if there was one or the residence of a provincial governor or military commander – were located somewhere within. In most cases, without proof of where the late Roman *praetoria* were located, Brühl discounts the site of the cathedral and likely early residence site of the bishop, and searches for late evidence of royal, ducal, or comital palaces, residences, or castles: the '*tours des comtes*' or *palais de justice*. In Brühl's (1977, 425) own words, the *palais de justice* is a leitmotiv of his work. These he takes to have developed from Merovingian, Carolingian, or Capetian royal palaces. The position is therefore assumed to have been that of a Frankish palace, which in turn is assumed to date ultimately to the Roman period. To this is added the assumption that the *praetoria* are most likely to be found backing onto the Roman wall and preferably in a corner of the wall's circuit and at the opposite side of the city to the episcopal palace and cathedral. The exceptional nature of those sites which do not conform to this spatial setting strengthen Brühl's belief that his method is correct.

Now while Brühl's methods provide the best system for postulating the likely positions of Merovingian royal, ducal, or comital residences in cities, we must be fully conscious that any discussion of continuity from the Roman period will be entirely self-fulfilling if based on Brühl's work. An external check on Brühl's prophecies is, alas, badly needed. This is made difficult by the fact that late imperial palaces and *praetoria* from Gaul are known archaeologically from only a few sites. The list consists of Trier, Arles, Köln, and Geneva. Proving continuity in these cases is an even greater task. As Wightman (1977, 304) said, 'I do not know any example of archaeologically proven continued occupation of a secular building beyond the first half of the fifth century.' This is a judgement that almost fits the above list.

Brühl (1977, 422) would like to add Senlis to the above list, but the evidence collated by himself (1975, 89) is based on his methodological assumptions described above; there is no solid archaeological evidence for the Roman *praetorium*. Arles, which boasts such an important role in the fifth century, yields the impressive upstanding remains of what are locally known as '*les thermes*'. Brühl (1975, 243) interprets the site as the imperial palace, but there is

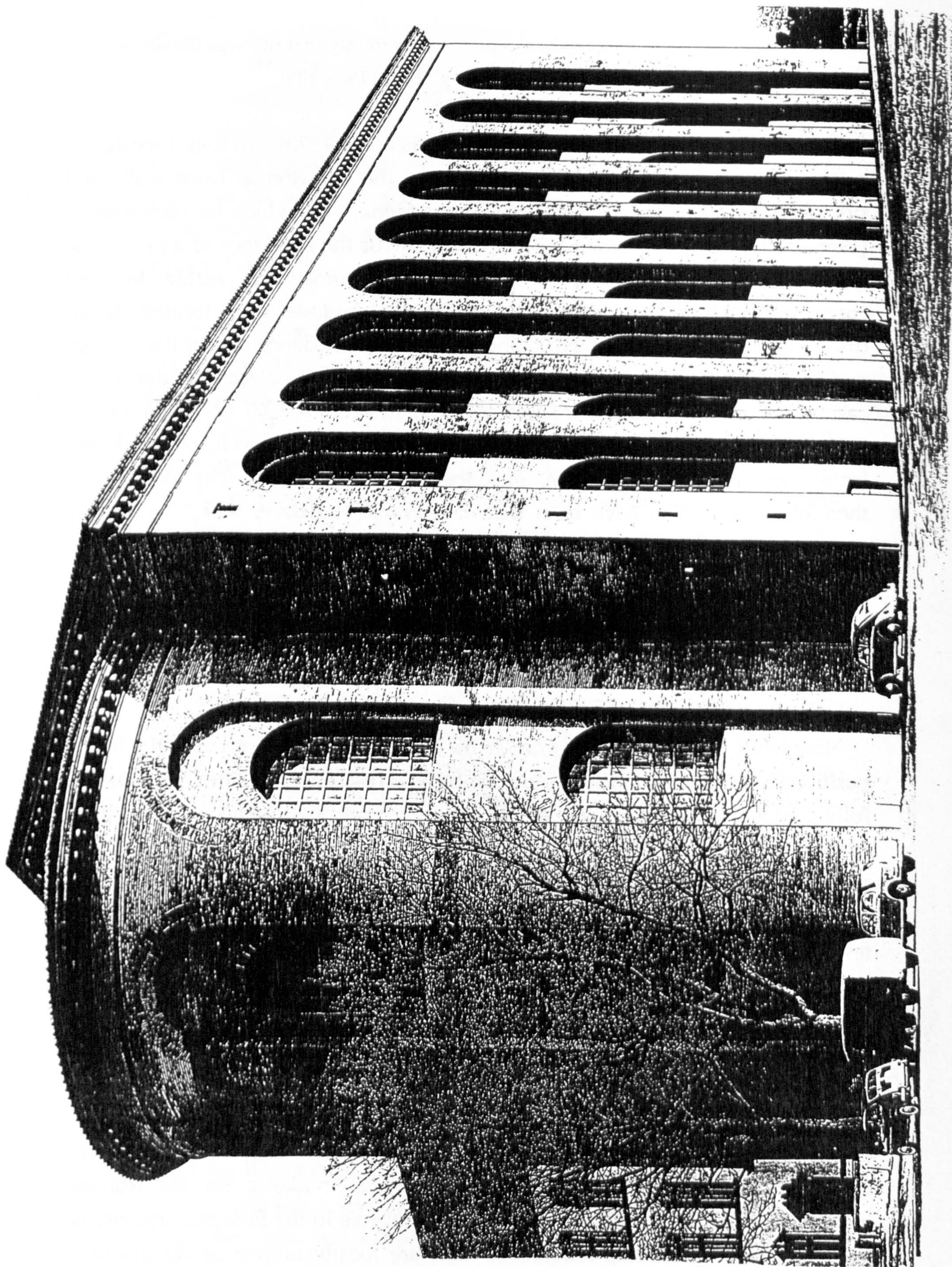


Fig. 4.1 The Basilika in Trier.

disagreement over its function. A sixth-century reference to the *palatium* (*Vita Caesarii* c. 29; 30) can reasonably be assumed to be that of the former imperial palace. And, whatever its function, if 'les thermes' have survived up to the present to the extent they do, they must have survived intact throughout the sixth century.

Trier. With its still upstanding Basilika (fig. 4.1), the imperial reception hall, Trier must have had more than one Roman palace; it is a question of how many (Brühl 1958, 252). There was perhaps more than one imperial palace, certainly a palace for the praetorian prefect of Gaul, and perhaps another for the provincial governor. For all its fourth century grandeur, Merovingian kings were not attracted to Trier, although a *palatium regis* is recorded (HF 10.29). It is assumed that the Basilika and the attendant imperial palace was taken over by the Merovingian kings, although no Frankish material has been discovered here (Böhner 1958, 291). It is inconceivable that the building could have survived beyond the Merovingian period without having been kept up by someone. But was that someone the Church? At some stage the Basilika became episcopal property. The tendency for bishops to take over state administrative functions in the Frankish realms was marked (Prinz 1973) (discussed in chapter two in relation to the maintenance of town walls). The Kaiserthermen were remodelled in late Antiquity, and it is thought the new structure served as an imperial residential or governor's palace. One is reminded of the baths/imperial palaces debate at Arles. It is sometimes further believed that the buildings were taken over as the count's palace. This would appear to be pure speculation.

There is also the likelihood that the municipal *horreum* was taken over as a royal palace. A later medieval church, at the nunnery of St. Irmina's, was built on the foundations of the *horreum* and arguably the earliest church simply reused the building (see chapter two). The foundation of the nunnery was made possible by the donation of a *palatium ad horreum* by King Dagobert. There is, therefore good reason to believe it once formed part of a palace complex, although whether functioning as *palatium*, *horreum*, or *oratorium* is impossible to say.

Bibliography: Brühl 1958; 1975; Eiden and Mylius 1949; Eiden 1952; Ewig 1954; Laufner 1964; *Trier Kaiserresidenz und Bischofsstadt* 1984.

Köln. The development of the Roman *praetorium* in Köln is well documented archaeologically, following excavation beneath the Rathaus. This was the

headquarters of the provincial administration on the east side of the Roman city and hard by the town walls overlooking the Rhine. The city *curia* is reckoned to have been in the forum, west of the *praetorium*. The final construction dates to after 309 and was truly a monumental building (fig. 4.2). It lacked heating and is thus considered to have only been used for special events. Within the octagonal 'reception hall' was a plinth which surely held a very large statue. Further to the south was found a second building, well-heated, which probably housed the offices and residence of the governor.

Doppelfeld (1970; 1973a; 1973b) is convinced that the buildings survived the Frankish conquest. A story by Gregory of Tours (VP 6.2) reveals the presence of a royal hall in Köln in which St. Gallus hid from angry pagans, although there is no guarantee that the building in question was the old Roman one. Steuer (1980) in a book dedicated to Frankish Köln, more or less ignores the question, for there is no positive evidence of continuity. The argument for continuity (discussed more fully in chapter two) put forward by Doppelfeld is simply this: we can date the late Roman construction of the *praetorium* but its use by the Merovingians would have left almost no archaeological trace. Such traces normally take the form of rubbish pits, burials, and covering layers of debris, by implication abandonment. The very absence of Merovingian material is therefore suggestive of continued use. The Carolingian date of refuse by the site and the siting of the medieval *aula regia* by the cathedral suggests to Doppelfeld (1973b, 33) a transferal in the ninth century.

Bibliography: Doppelfeld 1956; 1958; 1970; 1973a; 1973b; Precht 1973; Steuer 1980.

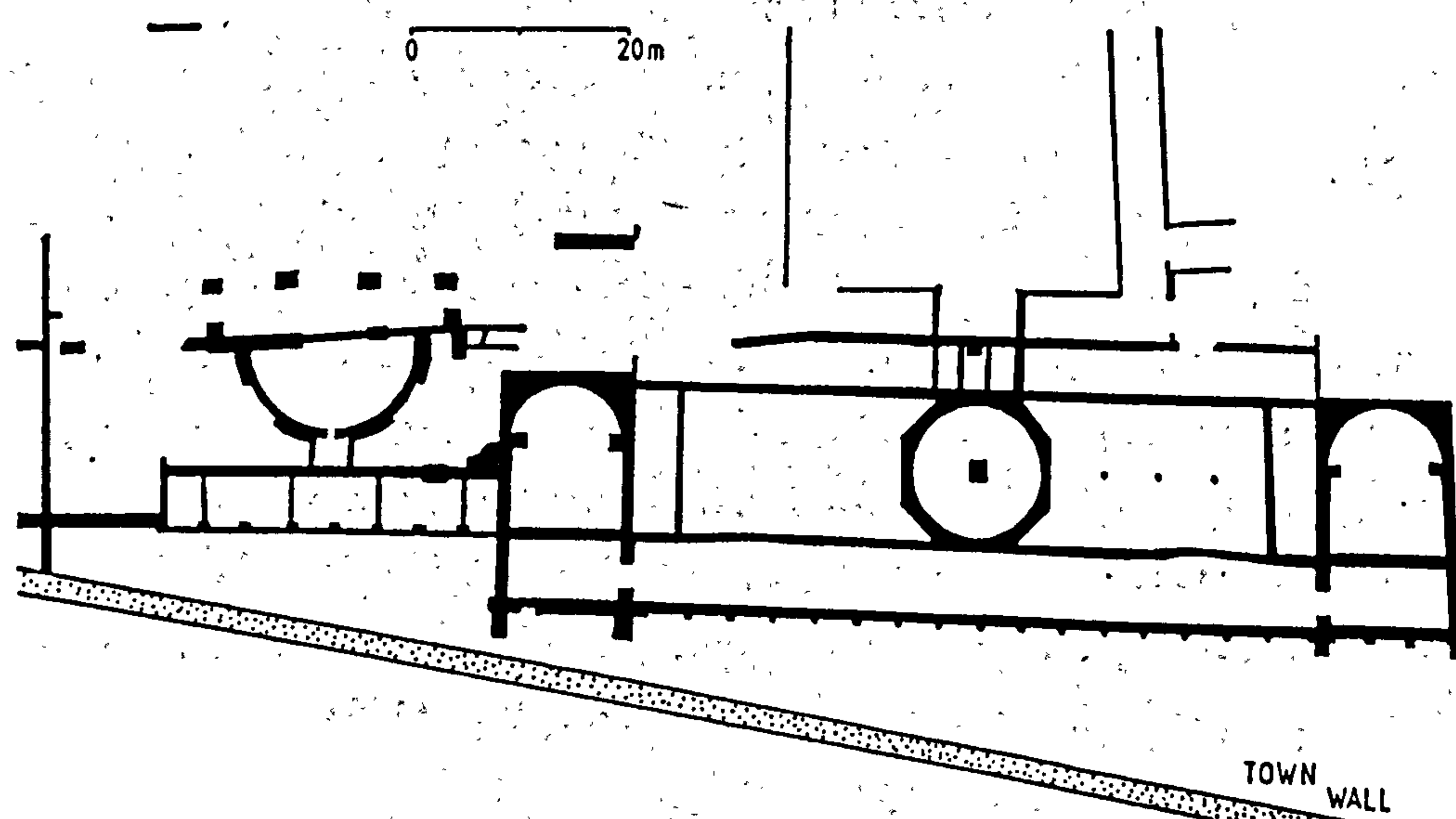


Fig. 4.2 The praetorium at Köln (after Precht), thought by Doppelfeld to have remained in occupation until the Carolingian period.

The late Roman *praetorium* in Geneva, if it is indeed the *praetorium*, appears to yield sufficiently good evidence of fifth-century use (fig. 2.3). Although of minor importance within the western empire (and as a result the size of the town and its monumental buildings were mediocre), Geneva became the *sedes* of the early Burgundian kings. Their expenditure on the town, such as Gundobad's work on the town walls (chapter two), may be responsible for better preservation of Roman buildings than in other towns.

The little archaeological evidence we have suggests that Roman buildings were indeed used as royal Merovingian urban palaces, while textual evidence gives us at least the hint that building in stone was the norm. The little evidence we have, however, does not tend to support Brühl's thesis. At Geneva the assumed *praetorium* lies by the cathedral and not in the corner of the town walls. At Trier the Basilika ended in episcopal hands rather than royal, and the municipal granary appears to have formed part of the royal palace. Neither was it always a Roman building that was used if we can believe Fredegar's continuator, who recorded the new construction of a palace in Bourges (*palacium sibi edificare iubet*) (Cont. 49).

Of no little interest is a comparison with the position in Italy, which is slightly better documented and recently brilliantly summarised by Ward-Perkins (1984). Maintenance of imperial palaces by Ostrogothic kings is presumed and in the case of the Palatine palace in Rome is documented by the Anonymus Valesianus who recorded (Ward-Perkins 1984, 159) that Theodoric 'ordered that two hundred pounds of gold from the wine fund be set aside for the repair of the palace and the restoration of the city's public buildings' and this despite the infrequency of royal Ostrogothic sojourns in Rome. The epitaph of Plato (d. 688), the father of Pope John VII, denotes him as *curator palatii*, seemingly that of the Palatine hill, the last recorded secular officer of a classical Roman public building. When Otto III established his palace in Rome, it was possibly to the long neglected Palatine that he turned.

The creation of three new palaces at Verona, Pavia, and Ravenna was attributed to Theodoric by contemporaries, although Brühl (1977, 422) suggests that the governor's residence must have served as the core, to which Theodoric added. At Verona the palace was situated in the *castrum*, a small area within the city walls set across the Adige on a prominent knoll. The Anonymus Valesianus stated (Ward-Perkins 1984, 160) that 'similarly at Verona... he built a palace, and added a portico all the way from the city gate to it' which Perkins interprets as designed to provide a suitable backdrop for royal arrivals and departures from the city. About the palace at Ravenna the Anonymous said, 'He fully completed a

palace but did not dedicate it. He built porticoes around it.'

Theodoric's palace (Ravenna). Alone among the Ostrogothic and Lombardic palaces, that of Ravenna has yielded some evidence of its architecture. There are three sources that provide us with information about the palace: the mosaic in the church of Sant'Apollinare, the account of Bishop Agnellus from his *Codex of the Bishops of Ravenna*, and summary excavation evidence.

Agnellus in the ninth century describes the palace at some length:

Here at Ravenna there was a similar mosaic [to that at Pavia] in the palace which he himself built. It was in the tribunal of the *triclinium* which is called *ad mare*, above the door, and on the facade of the palace of this city, which is called Ad Calchi, where the first door of the palace was, in the place which is called Sicrestum, approximately where the church of S. Salvatore now is. In the pinnacle of this same place there was a figure of Theodoric, beautifully made in mosaic, in his right hand holding a lance, in his left a shield, and wearing a breastplate. On the shield-side stood a personification in mosaic of Rome, with spear and helmet; on the lance-side was a similar figure of Ravenna, approaching the king with her right foot on the sea and her left on the dry land.

A representation of the palace would seem to exist in the mosaic PALATIUM on the walls of Sant'Apollinare in Ravenna, built by Theodoric (fig. 4.3). Even the mosaic of Theodoric described by Agnellus was probably once depicted on the Sant'Apollinare mosaic over the main entrance, although it and the Gothic statues within the front porticoes were removed at some point, perhaps when the church was rededicated by the Orthodox Church.

The interpretation of the architectural details has not proven to be free from debate. Three schools of thought exist. Dygvve (1959) believed that proper perspective had been abandoned and that the two side wings were intended to represent arcades flanking an approach to the main facade, and thus at ninety degrees to it, much as if one were looking down the nave of a roofless church. He suggests it is something like the passage leading up to the domed vestibule of Diocletian's palace at Split as an example, although his main aim was to reveal the intrinsic similarities between late Roman palaces and early Christian basilican churches. The Ravenna palace is, therefore, compared by Dygvve with St. Peter's in Rome. Noël Duval (1960) originally argued that the mosaic portrayed the entire front facade in proper perspective, although he later abandoned this and followed Lampl (1960-1), who revealed that there were two major early medieval architecture representational techniques, among other lesser ones. One was coined 'the split edifice'. 'To this end the edifice seems to have been split open along its longitudinal axis and each half folded sideways' (1960-1, 9). The other, 'the open arcade hall', is

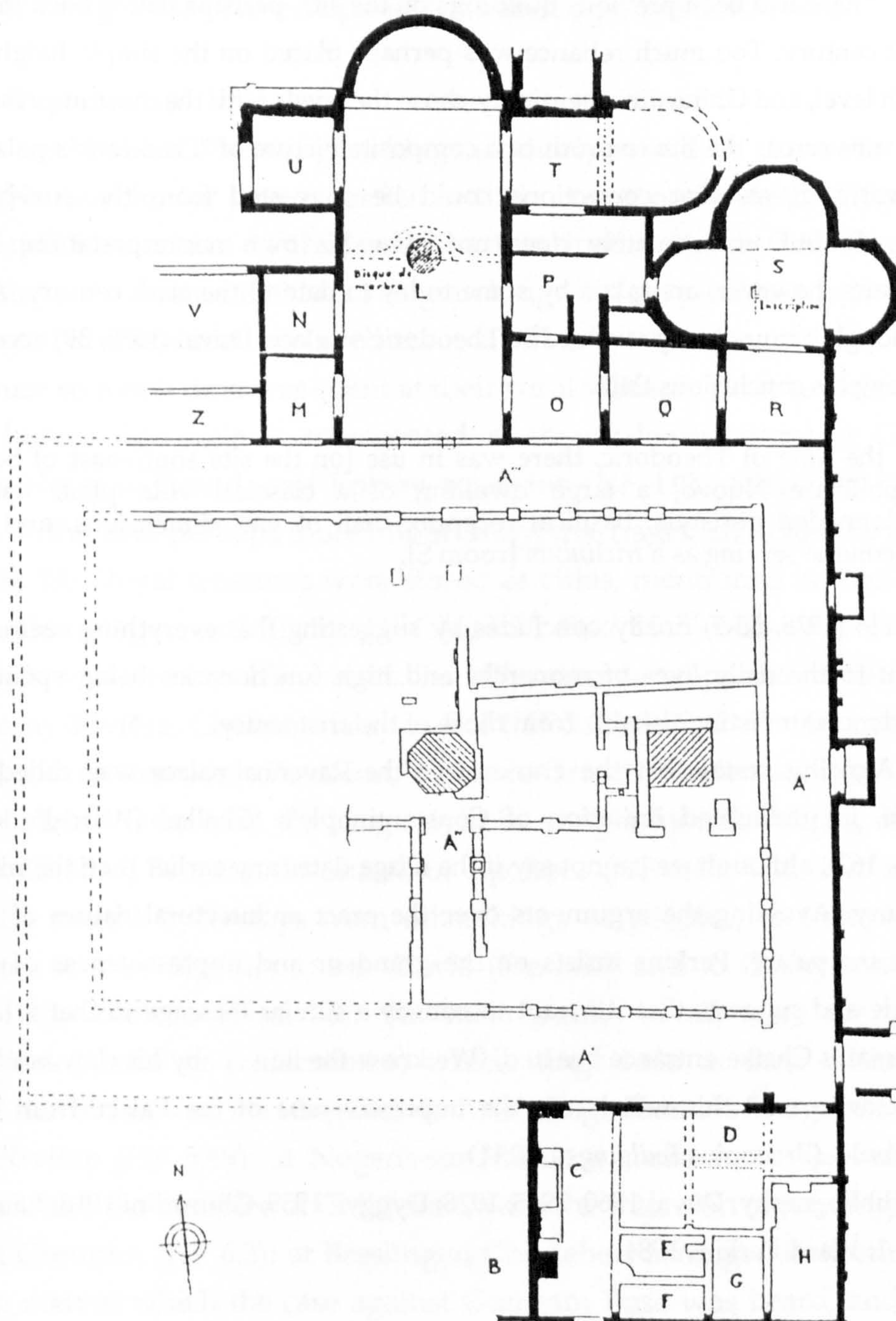
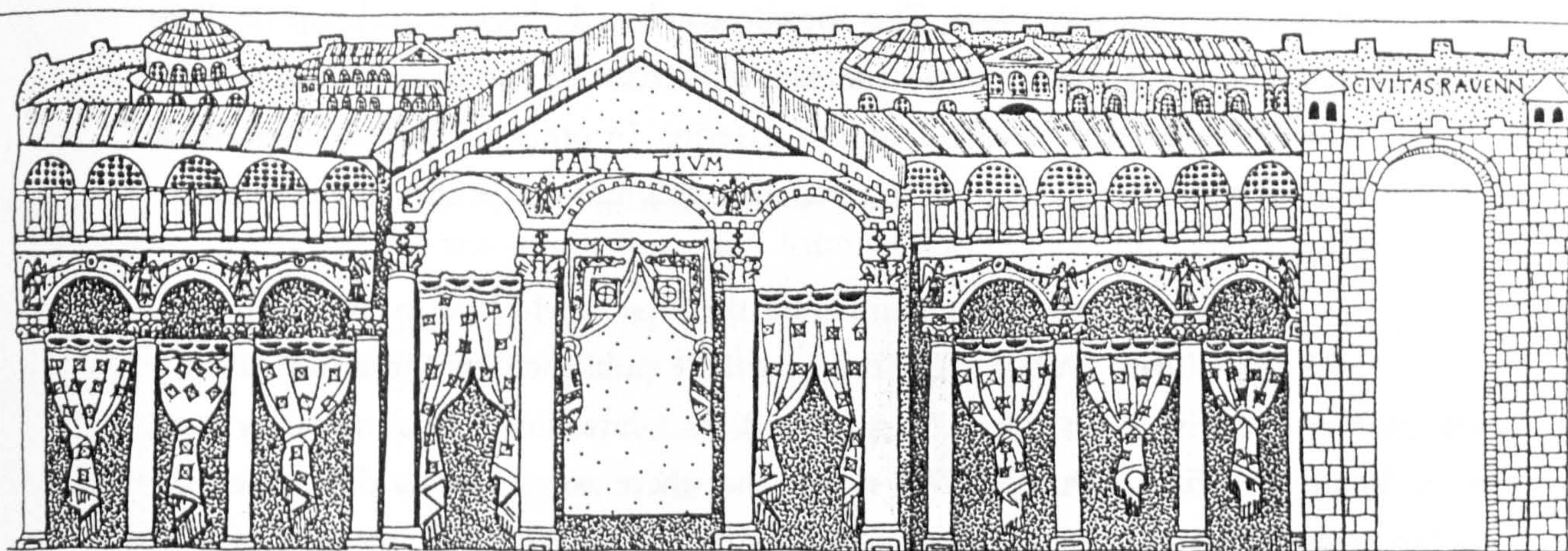


Fig. 4.3 The PALATIUM mosaic at San Apollinare Nuovo at Ravenna (drawing by author) and plan of the excavations of 'Theodoric's palace' (after Ghiardini).

in effect a cut-away view. The mosaic combined the two techniques. The resulting representation was of a basilican structure, the gable facade end of which was possibly arcaded on the exterior. In other words, the supposed portico wings are simply the internal aisle columns and arches of an ordinary three-aisled hall.

The excavations undertaken before the First World War to the south-east of St. Apollinare Nuovo were only partially published by Ghirardini (1918). The attractive plan (fig. 4.3) produced by Ghirardini is unfortunately not entirely reliable. Duval (1978) noted that there were serious difficulties in establishing chronological relationships between the various sections of the site. There had been previous buildings on the site, perhaps dating back to the first century. Too much reliance was perhaps placed on the simple height of each level, and Ghirardini seemingly chose the levels with the most impressive remains across the site to produce a composite picture of 'Theodoric's palace'. Duval suggests that corrections could be suggested from the surviving records, but unfortunately does not offer his own reinterpretation. The mosaics, however, are taken by some today to date to the sixth century. And although it cannot be proven to be Theodoric's palace, Duval (1978, 39) accepts among his conclusions that:

In the time of Theodoric, there was in use [on the site south-east of Sant' Apollinare Nuovo] a large dwelling of a classical villa plan, with colonnaded peristyle, basilical reception hall of vast dimensions, and a triconque serving as a *triclinium* [room S].

He (1978, 58-9) finally concludes by suggesting that everything seems to point to the daily lives of monarchs and high functionaries being spent in residences undistinguishable from those of the aristocracy.

Agnellus states that the entrance to the Ravenna palace was called *ad calchi*, in undoubted imitation of Constantinople's 'Chalke' (Ward-Perkins 1984, 162), although we cannot say if the usage dates any earlier than the ninth century. Avoiding the arguments over the exact architectural nature of the Ravenna palace, Perkins insists on the grandeur and impressiveness of the facade and suggests that it meant to convey a similar message to that which Justinian's Chalke entrance boasted, 'We know the lion . . . by his claw, and so those who read this will know the impressiveness of the Palace from the vestibule' (Procopius *Buildings* 1.10.11).

Bibliography: Duval 1960; 1965; 1978; Dyggve 1959; Ghirardini 1918; Lampl 1960-1; Ward-Perkins 1984.

We are extremely fortunate in having a statement of what functions palaces were intended to perform in Theodoric's correspondence through his secretary Cassiodorus (*Variae* 7.5):

Palaces are the delight of our power, the fine face of our rule, and the honoured witness of our kingship. Admiring ambassadors are shown the palace, and from their view of it they form their first impressions of the king. A thoughtful king therefore greatly enjoys a beautiful palace and relaxes his mind, tired out by public cares, in the pleasure of the building.

Perhaps one day, if archaeologists can ever shed enough light on the architecture of Merovingian villas, it may be conjectured that Byzantine or Ostrogothic forms were taken as sources of inspiration, just as Justinian's palace in Constantinople may have inspired Theoderic's palace in Ravenna.

Royal Residential Villas

We can assume that Merovingian kings similarly saw their domestic residences as suitable vehicles for the expression of their power and wealth. Although there is no explicit reference in the texts like that in the *Variae* (7.5), there is good circumstantial evidence that Merovingian kings did so, particularly in the form of the array of events which occurred at royal villas.

Because so much time was spent at their rural villas, it is not surprising that the whole human life-cycle is represented at them: deaths occur at Compiègne, Chelles, Noisy-le-Grand, and Épinay-sur-Seine (*HF* 4.21; 6.46; 5.39; *Fred.* 29), as well as births, and perhaps more importantly, marriages, as at Reuilly and Clichy (*Fred.* 58; 53). Royal treasures were stored at villas, mentioned at Chelles, Berny-Rivière, Compiègne, and Clichy (*HF* 6.46; 4.22; *Fred.* 85). Villas were also the stage for a wide variety of public events. To villas people were summoned or put on trial: Berny-Rivière, Chelles, Malay-le-Roi (*HF* 5.25; 5.39; *Fred.* 44), and the guilty imprisoned or executed: Ponthion, Noisy-le-Grand, Marlenheim (*HF* 4.23; 5.39; *Fred.* 43). Interviews with the king might be sought by individuals, as at Berny-Rivière, Chelles, Bruyères-le-Châtel, or Époisses (*HF* 4.46; 6.46; *Fred.* 36), or they might be sought by envoys from other kings or peoples; to Compiègne came magnates sent by Sigibert to see Clovis (*Fred.* 85) and to Clichy came Judicael, king of the Bretons (*Fred.* 78). But most public of all were the councils or assemblies (*synodi* or *conlocutiones*) which kings held at their villas. Chilperic invited the bishops of his kingdom to hear allegations made against Gregory at Berny-Rivière (*HF* 5.49); at Nogent-sur-Marne a conference was held between Chilperic and the notables of Childebert, with the intention of forming an alliance against Guntram (*HF* 6.3); at Breslingen Childebert arranged a meeting with his leading men at which the case against Guntram Boso was heard, and the queen

Table 4.2 Royal Merovingian Villas with Attested Visits

| Modern Name (département) | Latin Name (medieval source) | Secondary Source |
|--------------------------------------|--|--------------------------------------|
| 1. Andelot (Haute-Marne) | (HF 9.20) | |
| 2. Les Andelys (Eure) | (<i>V Chrothildis</i> 11) | |
| 3. Andernach | (<i>Fortunatus' Carm.</i> 10.9) | |
| 4. Arele (Eure) | <i>Arealo villa</i> (F 25) | |
| 5. Athies (Somme) | <i>Adteias villa regia</i> (<i>V Radegundis</i> c.2) | Agache 1983 |
| 6. Baizieux (Somme) | <i>Bacivo villa</i> (F Cont 2) | |
| 7. Berny-Rivière (Aisne) | <i>Brinnacus villa</i> (HF 4.22,46;5.25,34,39,50) <i>Bernaco villa publica</i> (F. Cont 37) | |
| 8. Bonneuil-s.-Marne (Val-de-Marne) | <i>Bonogillo villa</i> (F 44) | Roblin 1951 |
| 9. Bruyères-le-Châtel (Essonne) | <i>Brocariacum villa</i> (F 36) | |
| 10. Chatou (Yvelines) | <i>Captonnacum palatium publicum</i> (MGH DDMer 40,59; Spuria 6,7,8) | |
| 11. Chelles (Seine-et-Marne) | <i>Cala villa</i> (HF 5.39; 6.46) | Roblin 1951, Berthelier-Ajot 1986 |
| 12. Choisy-au-Bac (Oise) | <i>Cauciacum</i> (LHF 50) | Roblin 1978 |
| 13. Clichy (Haute-de-Seine) | <i>Clippiaco villa/palacium</i> (F 53,55,78) | Roblin 1951 |
| 14. Compiègne (Oise) | <i>Compendio villa</i> (F 26, 85; HF 4.21, 5.39) | Roblin 1978 |
| 15. Conflans-St-Honorine (Yvelines) | <i>castrum</i> (MGH DD Mer 92) | |
| 16. Crécy-en-Ponthieu (Somme) | <i>Crisciacus</i> (F Cont 2) | |
| 17. Creil (Oise) | (<i>V Eligi</i> 1.13) | Roblin 1978 |
| 18. Écry (now Asfeld) (Ardennes) | <i>Erchrego villa</i> (F Cont 36) | |
| 19. Épinay (Eure) | <i>Spinogelo villa</i> (F 79) | |
| 20. Époisses (Côte-d'Or) | <i>Spinsia villa</i> (F 36) | |
| 21. Étampes (Essonne) | <i>Stampas</i> (F 26) | |
| 22. Éterpigny (Somme) | <i>Sterpiniacum</i> (MGH DD Mer 10) | |
| 23. Étrépigny (Eure) | <i>Stirpiniacum</i> (MGH DD Mer 11, 38; Spuria 22) | |
| 24. Lagny-le-Sec (Oise) | | Ewig 1965, 154 |
| 25. Lagny-s.-Marne (Seine-et-Marne) | (<i>V Fursei</i> 9, 11) | |
| 26. Luzarches (Val-de-Marne) | <i>Lusarca palacium nostrum</i> (MGH DDMer 49,64) | Roblin 1951 |
| 27. Malay-le-roi (Yonne) | <i>Masolaco villa</i> (F 49, 79) | |
| 28. ? Mareuil ? | (<i>V Aunemundi</i> AA SS Sep 7) | |
| 29. Marlenheim (Bas-Rhin) | <i>Mariligium villa</i> (HF 9.38, 10.18) <i>Marolegia villa</i> (F 43) | Plath 1904 |
| 30. Mélicocq (Oise) | (<i>Gesta ss patrum Font. coen.</i> 4.2) | |
| 31. Montmacq (Oise) | <i>Mamaccas palatium nostrum</i> (MGH DD Mer 75, 77) | Roblin 1978 |
| 32. Nanteuil-le-Haudouin (Oise) | | |
| 33. ? Nemours (Seine-et-Marne) | <i>Nemausum</i> (MGH DD Mer 42) | |
| 34. Niederbesslingen | <i>Belsonacum villa</i> (HF 8.21) | |
| 35. Nogent-s.-Marne (Val-de-Marne) | <i>Novigentum villa</i> (HF 6.2, 6.3, 6.5) | Roblin 1951 |
| 36. Noisy-le-Grand (Seine-et-Oise) | <i>Nuceto villa</i> (HF 5.39) | Roblin 1951 |
| 37. Noyon (Oise) | (LHF 53) | |
| 38. Palaiseau (Essonne) | <i>Palatiolus</i> | Roblin 1951 |
| 39. Péronne (Somme) | <i>Perunna villa</i> (<i>V Radegundis</i> c. 11) | |
| 40. Ponthion (Marne) | <i>Ponticonem villa</i> (HF 4.23, 6.36) <i>Ponteugone villa publica</i> (F Cont 36) | |
| 41. Pont St-Maxence (Oise) | (LHF 45) | |
| 42. Quierzy (Aisne) | <i>Caratiaco villa palatii</i> (F 27, F Cont 24) | |
| 43. ? Reuilly or Romilly (Eure) | <i>Romiliaco villa</i> (F 58) | |
| 44. Rueil-Malmaison (Hauts-de-Seine) | <i>Rioilo villa</i> (F 3) | Roblin 1958 |
| 45. St-Jean-de-Losne (Côte-d'Or) | <i>Latona</i> (F 580) | |
| 46. Seltz (Bas-Rhin) | <i>Saloissa castrum</i> (F 37) | |
| 47. Thionville (Moselle) | <i>Theudone villa publica</i> (F Cont 36) | |
| 48. Vaudreuil (Eure) | <i>Rhodoialansis villa</i> (HF 7.19) | |
| 49. Valenciennes (Nord) | <i>Valencianae palatium regium</i> (MGH DDMer 66; Spuria 81) | |
| 50. Venette (Oise) | (<i>V Ansberti ep. Rot.</i> 28) | |
| 51. Ver-sur-Launette (Oise) | <i>Vernum villa</i> (MGH DD Mer 78) | |
| 52. Verberie (Oise) | <i>Vernbria villa</i> (F Cont 21) | Roblin 1978 |
| 53. Vitry-en-Artois (Pas-de-Calais) | <i>Victuriacus villa</i> (HF 4.51, 5.1, 6.41) | |

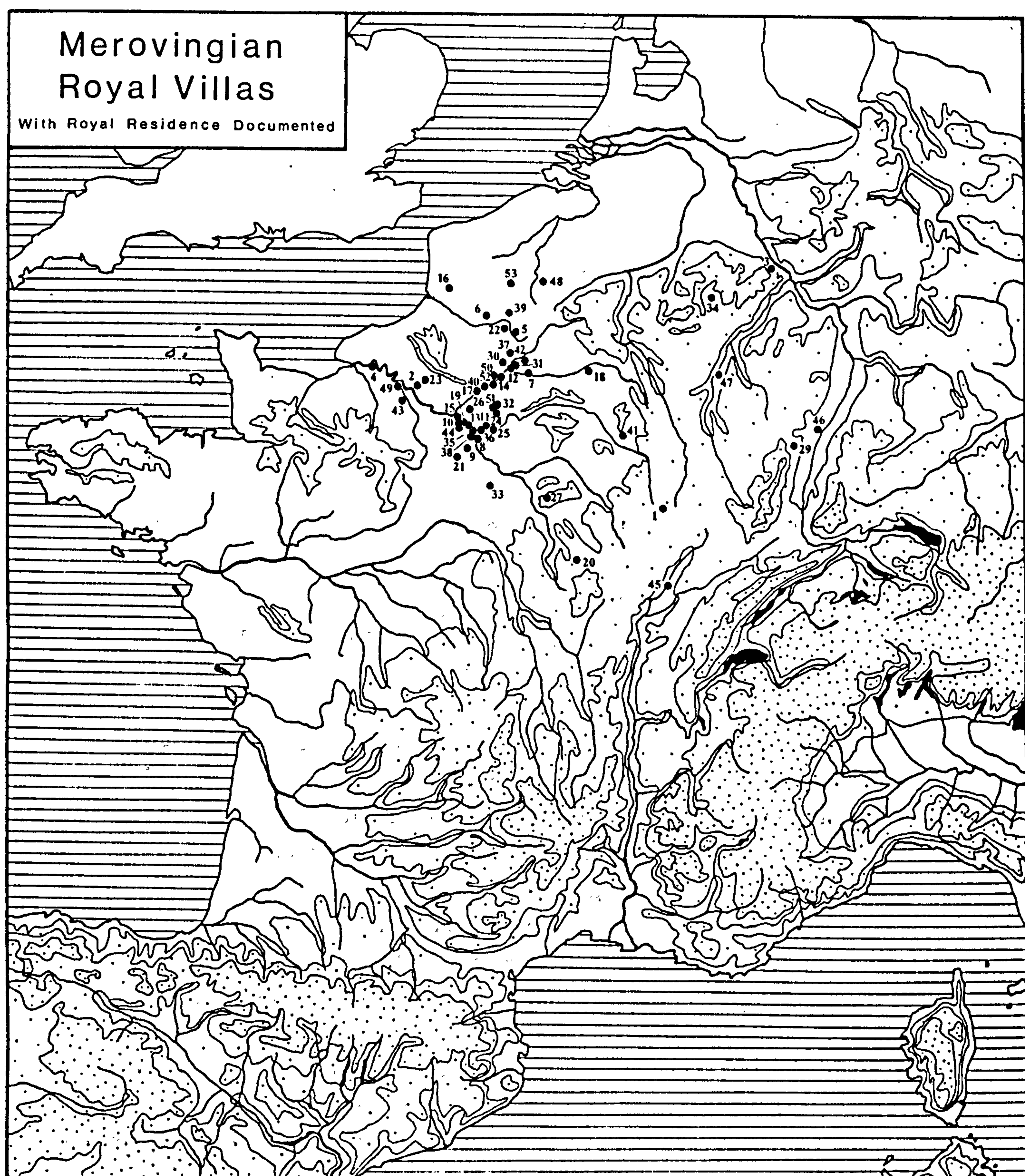


Fig. 4.4 Distribution map of the villas at which royal Merovingian residence is attested. Table 4.2 lists the medieval source of evidence for residence and secondary sources where some archaeological, historical, and topographic evidence is discussed.

pleaded on behalf of her daughter Ingund who was detained in Africa (HF 8.21); Lothar summoned the major domo and all the bishops of Burgundy to Bonneuil (Fred. 44); and Dagobert assembled all the great men and bishops of Neustria and Burgundy at Clichy to consider the country's problems (Fred. 55). Merovingian kings clearly saw these places as suitable settings in which to play out their role as the head of the kingdom, settings which would not detract from, but might indeed increase the majesty of their judgements and decrees.

When we seek archaeological or even topographical information concerning the villas on the map (fig. 4.4) we will find surprisingly little work has been done. The villas, it will be noticed, are concentrated heavily in the Paris basin. Thus it is that one third of the total appear in two works by Michel Roblin (1951, 1978), which review some of the départements in the Paris region for the Roman and Frankish periods. Surprisingly little information can be obtained about Merovingian villas in this region, despite the intensive work of Roblin.

The département of Oise was the object of Roblin's 1978 study. Here are located Choisy-au-Bac, Compiègne, Creil, Montmacq, and Verberie, some of the most important Merovingian villas. Of Verberie, Roblin (1978, 34) writes, 'we know nothing about the siting of the palace', and nothing more may be said about Montmacq. Although Compiègne has long been the object of several investigations, Roblin can only say that the site of the present-day château is not the original palace site. Choisy-au-Bac, which remained in the state's possession until the fourteenth century, has disappeared without leaving any visible remains. A find of 10,000 bronze Roman coins in the village implies Roman occupation and perhaps a villa. At Creil the microtoponymy suggests a site downstream from the sixteenth century palace, and one of much poorer defensive qualities than its later counterpart.

Bonneuil, Chatou, Chelles, Clichy, Luzarches, Nogent-sur-Marne, Noisy-le-Grand, and Rueil are situated within Roblin's (1951) earlier area of investigation. Of these we learn that Clichy is totally lacking in Roman finds, but that Roblin assumes that the parish of Rouvray-Clichy at 2000 ha. represents the original extent of the Merovingian estate. Luzarches lies on a Roman road and 140 *Reihengräber* have been excavated at the nearby site Noyer à la Drouarde. The church dedicated to saints Côme and Damien in Luzarches had rights over four other village churches: this is held to reveal an original estate. Similarly at Bonneuil a supposed great 'primitive parish' is reconstructed from church dedications, although this involves the chronological disentanglement of three separate dedications to St. Martin. The result is somewhat suspect. Noisy-le-Grand has a church dedicated to Our Lady and St. Sulpicius and therefore,

according to Roblin (1951, 315-6), 'cannot' date until after 644. If this were so it would imply that Noisy-le-Grand did not have a church when it appeared in the writings of Gregory of Tours. Is it likely? Nogent-sur-Marne lies on the slopes of Belleville plateau commanding the second loop of the Marne and a near-by site has yielded a Merovingian cemetery. Although the dedication of the present-day church would not allow it to date before the ninth century, Fortunatus called Nogent a *vicus*, implying the existence of a parish church. Given the propensity for church dedications to be changed, it is quite possible that the Carolingian dedications at Noisy and Nogent were not the original ones, and that both had Merovingian churches.

Chelles (Seine-et-Marne). The centre of Chelles, less than a kilometre north of the banks of the Marne, occupying a position at the branching of Roman roads, has yielded evidence of Roman occupation, including the foundations of stone walls. Of particular interest is the site of the 'Cour-du-Palais-Royal' (fig. 4.5). Here the stone walling, of careful ashlar and associated with pieces of black and white mosaic (probably late Roman), was overlain by a supposed Merovingian occupation layer (Nadine 1986). Nearby is the site of the 'Palais-des-Tournelles' which has yielded roof-tiles of Roman form and a section of wall built of large ashlar blocks of uncertain date. It also lies on the road 'Pont-St-Martin' and just here was, almost certainly, a chapel dedicated to St. Martin which disappeared in the seventeenth century. On the south end of the site *sépultures de plâtre* have been discovered. These are sarcophagi of gypsum plaster and probably derived from a Merovingian cemetery, thus tempting us to interpret it as the site of a royal chapel in the heart of the Merovingian villa, recognisable today only from the telltale place-names.

Chelles is first documented by Gregory of Tours (*HF* 5.39; 6.46; 7.4; 10.19) when it appears as one of King Chilperic's most important villas. In two anecdotes Chilperic is found hunting and, returning from his final hunt, he was assassinated here. In no less than three of the four anecdotes we hear about Chilperic's treasure, part of which Chilperic's wife, Fredegund, removed to Paris. Saint Géry visited Lothar II at Chelles sometime between 604 and 613 (*V sancti Gaugerii*). He spent a night in prayer there in a church. A palace is not mentioned again for nearly four centuries, although Charlemagne, Louis the Pious, and Charles the Bald all visited Chelles. It need not be assumed that they were staying at their own palace, for it is probable that they were guests of the very important abbey which existed at Chelles until the Revolution.

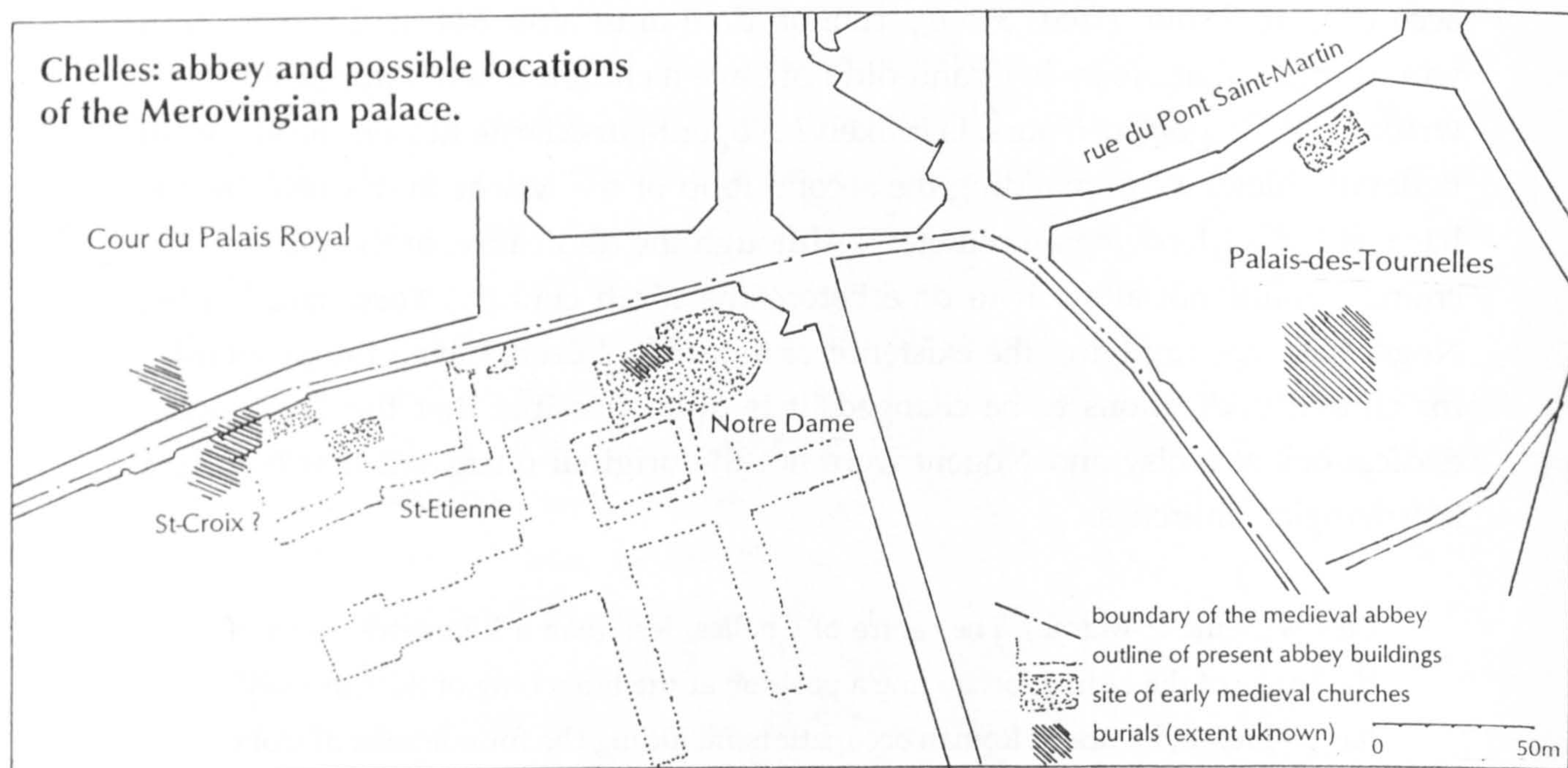


Fig. 4.5 Plan of early medieval burials, abbey site, and possible sites of the royal palace based on place-name evidence.

A nunnery was founded at Chelles, according to the *Life of Bathild*, by Clovis I's widow, Clothild, thus between 511 and 545. It is a wonder that Gregory of Tours makes no mention of it. Berthelmer-Ajot (1986, 359) similarly notes this peculiarity and suggests that it had perhaps dwindled away to insignificance. As Bathild was herself widow of Clovis II, there may be an element of exaggeration if not fabrication in the attribution to Clothild. If not, it would mean that at the foremost of all the royal Merovingian villas, there was both palace and abbey.

The seventh-century multiplication of churches at the nunnery and monastery makes it difficult to pinpoint their position today. Berthelmer-Ajot suggests some locations by noting the discovery of plaster sarcophagi at various church sites in the town centre and, somewhat further afield, near the church of St. Andrew at the 'Montagne-de-Chelles'. She concludes that Chelles was composed of palace, monastic houses, and village, all as distinct units. I would prefer to see the monastic centre at Chelles as having taken over the royal villa as the gift of Bathild. Ewig (1965) shows how royal estates which explicitly divided the land between palace and new monastic foundations in the early Carolingian period, quickly became exclusively monastic centres.

Bibliography: Roblin 1951; Berthelmer-Ajot 1986.

Outside the royal Merovingian villas which fall into Roblin's study areas,

almost none have been the object of investigation. Marlenheim is one exception, although the most valuable information we do have of the site comes from contemporary documents, not from archaeological investigation.

Marlenheim (Bas Rhin). Marlenheim lies by the Mossig stream at the foot of the south-facing hill Marlberg, which is today covered in vineyards (fig. 4.17). Through this valley ran an important Roman road to Strasburg. Two pieces of Roman sculpture have come from the centre of the village. In a very small-scale excavation Plath (1904) tried to locate the Merovingian royal villa in the neighbourhood of the church, which sits on a slight rise, but found nothing.

Gregory of Tours (HF 9.38; 10.18) mentions Marlenheim twice. A certain Septimania was punished by disfigurement and sent to Marilegum villa 'to turn the mill and grind corn each day', while King Childebert fortunately spied assassins awaiting him, as he entered the oratory at his residence in Marlenheim (*in oratorium domus Mariligensis ingrederetur*). In 613, according to Fredegar (5.43), King Lothar and his wife Bertetrude went to the *villa Marolegia* where they executed those found guilty of killing Duke Herpo. Marlenheim remained in royal hands, even after the change in dynasty, and was visited by Louis the Pious.

Plath was very probably correct in suspecting that the Merovingian villa lay in the vicinity of the present day church. It is quite possible that the church represents a descendant of the private royal chapel mentioned by Gregory. There is also the possibility that Marlenheim was the site of a Roman villa, although such a speculation should not be pushed too far, given the paucity of Roman remains. The Merovingian villa, in any case, was sited just where one might expect a Roman villa: near an important Roman road, although set some way back from it, on a rise and overlooking a valley. And like Roman villas it was poorly situated for defensive purposes.

Bibliography: Plath 1904; Müller-Kehlen 1973.

Athies (Somme). A Merovingian villa was attested as the place of a long stay by St. Radegund before she became the wife of King Lothar I (*Vita Radegundis* 1.22). A Roman villa has been partially excavated some few hundred metres from the modern village of Athies. Dating evidence seems to suggest that occupation did not continue into the late empire. The village lies off the flat plain in the small valley created by a little stream. It is tempting to think that a fourth-century establishment replaced the excavated villa and that it was taken over by the Merovingians, whose villa ultimately gave birth to the modern

village. The Roman villa was first recognised by Agache's aerial photographic work. The absence of any other crop marks which might represent the Merovingian villa strengthens the belief that it underlies the modern village. Excavation is presently being undertaken in the village in search of the Merovingian settlement.

Bibliography: Agache 1983.

Le Mesge (Somme). Agache (1983, 26) notes that nearby Le Mesge offers a similar situation. Here a very large Roman villa can be detected some one or two hundred metres from the centre of the modern village which lies several metres lower in a very small valley. Le Mesge, although not attested as a residence, was a royal villa donated by Childebert III to Amiens cathedral.

Carouge (canton Geneva, Switzerland). Just across a bridge over the Arve leading from Geneva, where modern roads branch, lies a field by the name of Carouge, from which Roman antiquities have long been recovered. There is seemingly no difficulty in seeing a derivation of this place-name from *Quadrivium*, 'cross-roads'. Documentary sources reveal that Sigismund was crowned king of the Burgundians in 516 AD in the presence of his father at *Quadrivium*. The probability seems good that we can equate the two, especially in light of the survival of a Burgundian palace in Geneva itself and its importance as a *sedes*; the likelihood is strengthened by the analogy with Paris, a royal 'capital' surrounded by important royal villas.

Late medieval documents record '*un grand talus ou terreau, que précédait un fossé défendu par des barrières*' (Blondel 1940, 66). Originally Blondel thought it marked the limit of the forest of Pinchat, but it soon became apparent that it extended far beyond the forest limits and seems to enclose Carouge and some considerable area of land bounded by this ditch and the bank of the Arve. In 1932 excavation at the place d'Armes revealed '*une lignée de pilotis avec planches latérales indiquant un canal ou cours d'eau . . . Les depots indiquaient une periode voisine de l'époque romaine*' (ibid 66). Blondel interprets this ditch as the work of the Burgundian royalty, enclosing the villa once presumably owned by Romans, but gained through *hospitalitas*.

Bibliography: Blondel 1940, Fournier 1978.

Another major Merovingian villa to have attracted some scholarly attention is Quierzy, already discussed in the previous chapter. As we saw then, there is nothing which we may extract from the excavation reports which we can

confidently pinpoint in the post-Roman centuries before the late ninth century. It is unfortunate that Chapelot and Fossier (1985, 47) should casually claim that Roger Agache believes that he has aurally photographed the crop marks representing the Merovingian-Carolingian villa site without giving a reference or fuller discussion, particularly given the uniqueness of such a find and the importance of such villas to the topic of their book.¹

Of Quierzy, however, we can say that a Roman villa, if not two, were predecessors of the Merovingian and Carolingian villas. Although direct continuity cannot be proven, there are the very suggestive facts that a Gothic monastic enclosure exactly overlay the enclosure of a Roman villa rustica and that a ninth- tenth-century defensive enclosure enclosed the domestic range of a Roman villa very neatly. The faintest hint of a Roman villa precursor was suggested for Chelles, Choisy-au-Bac, and Marlenheim, but is incontrovertible for Carouge.

The evidence of royal Merovingian rural villas, such as we have, cannot be taken as proof that Roman villa buildings had been kept up and remained as the core. This might be a working hypothesis for the work to be done in Athies village. If Quierzy had been better excavated or had the finds at least survived, we might have had positive proof of continuity at such a level. Pfalz on the other hand seems to prove that the buildings only survived in an incomplete state. Such site continuity might well be illustrated in the future, if French archaeologists were to initiate a research programme designed to investigate such sites. At the moment we are left with only tantalising hints.

Some Other Merovingian and Gothic Villas

The overwhelming concentration on royal villas in this chapter is largely the result of the sources available. While named villas abound in the charters of the period, many owned by lay and ecclesiastical nobles, such as Bishop Bertram of LeMans, or even by small landowners, we cannot know whether such villas were residential or simply agricultural. Few of the really important royal Merovingian palaces appear in charters as gifts until after they were superseded or perhaps in bad repair. The inference we might draw is that donated villas were primarily agricultural in function, or perhaps crumbling, former residential villas. It would therefore be difficult to produce a list like table 4.2 of lay residential villas.

For these we must turn to the few exceptional, well-documented villas of the period. Like the villas of the fifth century, those of the sixth are known primarily

¹ I was unable to get further information from Roger Agache concerning this alleged photograph.

from documentary descriptions, foremost of which is the testimony of the Italian Venantius Fortunatus, bishop of Nantes, who offers us various descriptions of villas: of Nicetius, bishop of Trier, of Pontius (a descendant of Pontius Leontius), bishop of Bordeaux, and unspecified owners. I begin with these important poems before dealing with a few excavated sites. The very paucity of good excavation will be apparent if only from the fact that two of the three examples come from Visigothic Spain.

Bissonum (Carmina 1.18):

There is a place,
Where the restored field grows green with assiduous flowers,
Arable lands breathe out painted golden colours,
Vegetation emits fragrance to please in a friendly fashion.
The inhabitant of Bissonum calls this place by its former name.
It is seven miles from Bordeaux;
The happy possessor located here the welcoming *praetoria*,
The plain enduring a porticus at three places.

It [age] ruined the beautiful appearance of its face:
The work of this Leontius returns it to a better road.

.....
Meanwhile he restored new baths in the ancient manner,
Where weary men can have recreation at the enticing tub.

Vereginis (Carmina 1.19):

Amongst the fruitful fertility through which the Garonne's water twists,
The pleasant field shows signs of spring on the banks of Vereginis:
Here a short gentle ascent climbs to a mounded slope,

.....
The summit rises (*superbit*) neither too low nor too high.
In the middle of the hill sat the house (*domus*), becomingly built,
Cuius utrumque latus hinc iacet, inde tumet.
The proud machine ? of the house (*casae*) is suspended on a triple arcade,
Quo pelagi pictas currere credis aquas.
Concealed waters, begotten alive from metal ? spring forth,
Soft and wet perennial fountain of water;
Where, at the table up above, the pastor celebrates at the banquet.
Inclusoque lacu pisce natante bibit.
Now it comes to be renovated by the wages of father Leontius,
Which lord for a long time *culta* desired.

Praemiacum (Carmina 1.20):

... if the fourth syllable is cut off
Powerful Praemiacum has the name *praemia*.

.....
This house (*domus*) is founded where a flattish swelling ends in a rise,
The summit reigns from a not very elevated crown;
Lying above, the place slopes down to the stream,
Flowery meadows grow green under the jewel-adorned grass:

.....
Here from other parts ? the corn-fields grow with ears of corn,
Fruitful and nourishing vine-shoots shade the ground.

The waters of the Garonne are not failing in innumerable fish,

.....
 Although so many services were sought of you, Leontius:
 You were only lacking in those many good things you had already given away.
 Because your home (*domus*) is beautiful and your welcome baths shine,
Consolidatorem te cecinere suum.

A Wooden House (De domo lignea) (Carmina 9.15):

Away from here, you wall of Paros stone:
 I prefer with reason the wood of the artisan to you.
 The heavens vibrate his massive *palatia* of planks,
 Built by hand so that no gap is showing.
 All that which binds stone; sand, chalk, clay,
 Favoured woods alone builds the edifice.
 A high severe and square *porticus* surrounds it,
 And sculptured, it [the *porticus*] plays in the workman's craft.

Mediolanum (Carmina 3.12):

A mountain grows up in a precipitously hanging mass
 And raises its lofty head on the rocky bank:
 The leafy point lifts up from exposed rocks,
 The apex, well protected, reigns from the lofty summit.
 The hills advance while meadows recede in valleys:
 On all sides the lesser land falls away as it rises up;
 The proud Moselle and the smaller Dhron encircle it,
 And fight to provide this place with their fish.
 The waves of the river that otherwise ravish the pleasant produce of the earth,
 For you, Mediolanum, they teem with food.
 As the waters grow the neighbourhood offers up fish;
 It presents dishes of food whence otherwise comes rapine.
 The thankful inhabitant cleaves the fruitful furrows,
 Making offerings of heavy fertile corn-fields:
 Farmers feed their eyes with the future harvest,
 Before it reaps in sight what work the year has done.
 The pleasant field laughs, hidden in green growing grass,
 Soft meadows amuse wandering spirits.
 This pious man Nicetius therefore, wandering the countryside,
 As shepherd, built his desire for his flock:
 He girdled the hill with three tens of towers,
 Presented this construction where none had been before.
 From the summit branches of the walls were lowered,
 The terminus reached the Moselle's waters.
 The constructed hall (*aula*) shined from the rocky summit,
 And this home (*domus*) was a mountain set on another mountain.
 He wanted to enclose a wide area with a wall,
 And this house (*casa*) alone nearly formed a *castellum*.
 The lofty hall (*aula*) was held on marble columns,
 Which hall *super* summer-quarters, separated pontoons in the stream.
 The stretched out fabric, in rows of three were created,
 So that after you ascend, you can ponder the covered acres.

.....
 It is the place of the chapel and holds the arms of men,
 Also there is a twin *ballista* of flight,

.....
 Water is led in wet sinuous conduits,
 From which the violent watermill here produces food for the people.
 He introduced seductive grapes to the senseless hills,
 Cultivated vines grow where once was shrub.

Native fruit plants grow everywhere,
 They feed the place with a variety of flowery odours.
 They benefit you yet however much we praise them,
 So much good you grant, richly, pastor of your flock.

Mediolanum (Bernkastel-Wittlich, Rheinland-Pfalz). The site of *Mediolanum*, surprisingly, has never been identified on the ground with one hundred per cent certainty, although several attempts have been made. Kurt Böhner (1958) sites *Mediolanum* by the confluence of the Mosel and Dhron. Konrad Weidemann (1977) has followed this suggestion, complete with a map pinpointing where Nicetius's castellum and enclosure walls ought to be (fig. 4.6). The major stumbling block to this hypothesised site of *Mediolanum*, to my mind, is the equation of the Dhron with the *parvulus Rhodanus*. Ausonius mentions a *Drahonus* which must be the Dhron; *Rhodanus* seems an impossible etymological successor. The worries are intensified by the fact that Fortunatus was well acquainted with the poems of Ausonius, indeed he composed in the same style. I offer the possibility that Fortunatus was making a play on words, presenting the Dhron as a 'little Rhône' with *Rhodanus* as an anagram of *Drahonus*.²

If his suggestion is correct (and I believe it is roughly the right location), it is very interesting that numerous Roman remains have been found on the promontory around Niederemmel. Remains have come from under the parish church, dedicated to St. Martin, at Niederemmel, while spread over a wide area around the village, Roman pottery, brick, cement and building foundations were recorded in 1903. From east of the village came sarcophagi, in one of which was found a well-known glass vessel of '*Diatretglas*' (no. 60 in *Gallien in der Spätantike*) – a very luxurious and presumed expensive fourth-century product. For Böhner this speaks of the existence of a Roman villa. Roman remains continue on the hill overlooking Niederemmel. In the middle of the nineteenth century, '100 small square rooms', each of 'enough room for two or three men', were found lying in a straight line along a 'Roman road.' They were interpreted then as small shops or inns. Böhner says that if these were remains of Nicetius' 'Burg', then nothing remains today. Böhner fails to

² In the early seventeenth century, Brower suggested Bischoffstein, near Koblenz, near which ran the little river Rhon, which would be altogether more acceptable as a derivative of *Rhodanus* (Nisard 1887, *Fortunati Carmina* 3.12, footnote 2). However, today the only stream which runs by Bischoffstein is the Krebsbach (the name of the stream may have changed, although this seems unlikely). Today there are only the remains of a medieval castle perched on its rocky nest commanding the Moselle and the site is clearly not appropriate to a Merovingian villa given the findings of this chapter. There is, furthermore, no room for the numerous inhabitants described by Fortunatus.

mention the obvious: the description fits a Roman cemetery along a road, not a series of shops and inns. The site seems more likely to have been a *vicus* than a villa, in which case the walls and towers described by Fortunatus may well have been Roman and not Merovingian.

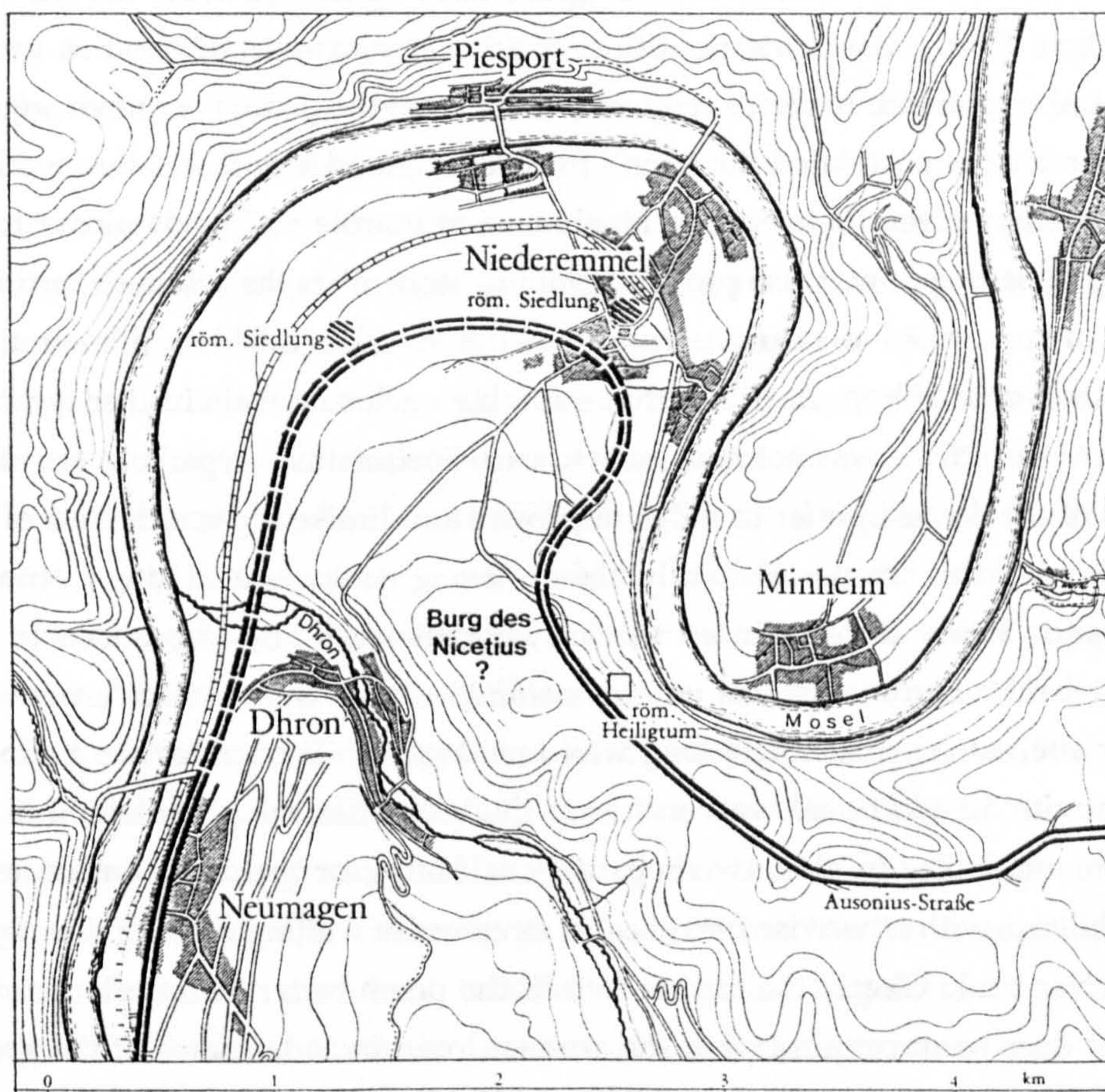


Fig. 4.6 The probable site of Mediolanum (after K. Weidemann).

Given Fortunatus's description, it is worth trying to visualise the site of Mediolanum if only to make clear how difficult such literary evidence is to use. For Böhner the real key to understanding the site is that Fortunatus describes an upper and a lower site. The former is supposed to be the fortification with 30 towers which Nicetius had built for the latter, a dependent and pre-existing settlement. Böhner argues that the house (*domus*) Nicetius had built was in the lower settlement for the boats one was meant to see on the Mosel from it, would not have been visible from the hilltop. This a feeble argument. Fortunatus may have been using poetic licence and in any case, provided the dwelling were anywhere short of the very summit of the promontory, boats are readily visible today, and Böhner is perhaps not

counting on the *aula* as being two or three-storied. Moreover, *contra* Böhner the poem clearly depicts the *aula* as, 'shining from a rocky summit' (*aula tamen nituit constructa cacumine rupis*), and the *domus* appears as 'a mountain on another mountain' (*et monti inposito mons erit ipsa domus*). Böhner's treatment of this text, as intractable as the rest of the poems of Fortunatus, is not always trustworthy.

The following information, I suggest, can be gathered about the site from the text. Firstly, the domestic residence (*domus, casa, aula*) of Nicetius was of central importance to the settlement (which from the poem we cannot suggest either pre- or post-dated Nicetius's foundation), and that it was designed for ostentation, given the evidence for the use of marble and its dominant topographic setting. It was also probably built of stone. Was the *aula* itself fortified? Fortunatus called it nearly a *castellum*, but he also called it 'a mountain'. Perched on a hilltop, clearly within a mighty enclosing wall, further fortification of the edifice was not necessary to earn Fortunatus's hyperbole. Fortification of the *domus* I prefer to judge unproven and unlikely given the rest of the findings of this chapter. Secondly, the enclosing wall was a striking feature of the site. 'Thirty towers girded the hill', but the wall also 'descended to the river bank'. Fortunatus was not so meticulous that we can decide between four alternatives: one single long wall forming a unitary enclosure; a similar plan with an additional wall enclosing the hilltop, forming something like a motte and bailey in plan; two separate enclosures; or the single enclosure of the hilltop, with otherwise inexplicable stretches of walling running directly to the river bank. Clearly the former two fit the poem better than the latter two. What does seem certain is that the area enclosed included some of the green fields that feature in the poem. Fortunatus explicitly states that Nicetius 'wanted to enclose a large area with a wall' (*conplacuit latum muro concludere campum*). This line unfortunately is separated from his four lines that discuss the massive enclosing wall by two lines that discuss the *domus* he had built. Moreover, this line is followed by two more dedicated to the *domus*. Böhner takes the logical position that the line is more closely connected with the dwelling and therefore refers to the *domus* itself. I interpret it to refer to the wall with 30 towers which branched to the river bank, and suggest that Böhner's interpretations demand too precise and accurate descriptions from the impenetrable Fortunatus. Thirdly and finally to be gleaned from the text, within the enclosure wall (which I presume to be of stone) there would seem to have been: farmers (*agricolae*) and inhabitants (*vicinius*); a chapel apparently set into a tower where arms were stored and a ballista stood; and a mill which

ground the inhabitants' flour with conduits carrying the water that powered it.

The picture painted is one of a considerable number of dependants, perhaps all living within the confines of a huge enclosure wall, overlooked by the ostentatious *domus* of the proprietor, Nicetius. They worked the fields which appear to have belonged to Mediolanum and fished the river.

Bibliography: Böhner 1958; K. Weidemann 1977.

Camp de Larina (Isère). Before joining the Ain and flowing into Lyon, the Rhône snakes north around the Ile Crémieu, a roughly 20 by 10 kilometre limestone plateau which rises abruptly over the Rhone plain to the west. The site is situated on the edge of the plateau above Hières-sur-Amby at the point where the Amby cuts a gorge through the limestone at an angle to its western face to join the Rhône. The resulting formation, on which the site is located, is a triangular promontory with nearly sheer faces of hundreds of feet on two sides. Merovingian occupation on the site reused an Iron Age hillfort (fig. 4.7). The local topography of the site did not allow for a simple 'barred spur' or 'promontory fort' rampart, so that only the cliff face overlooking the Rhône was used as one limit of the enclosure. The remainder of the circuit completed an overall D-shaped enclosure, utilising locally the marked natural heights, so that no positive identification of ramparts has been made for almost half of the circuit, and such ramparts possibly never existed. Thus an artificial rampart can be traced along some 950 metres of the total 1500 metre circuit, not counting the cliff edge, enclosing an area of 21 ha. The Iron Age rampart is poorly dated, but the final rebuilding included fragments of a Roman monument, believed to be the remains of a temple, and can be said to date probably to the Merovingian period. One small test trench to the south-east revealed a drystone wall still standing to over 3 metres, while the major section to the south reveals the base of the Merovingian wall, cut into the Iron Age rampart, and composed of stone bound with chalk mortar. The excavator suggests that the Merovingian circuit was not necessarily continuous, and may just have patched up particularly weak points of the Iron Age rampart circuit, but in view of the extremely limited investigation undertaken to date the suggestion remains hypothetical.

Within the enclosure there are four distinct areas of activity archaeologically demonstrated. Along the eastern wall, overlooked by a hillock just to the west, is an area which has not yet been investigated but has revealed a series of stone walls, thought to be tumbled drystone huts and interpreted as a 'hamlet'. Just beyond the enclosure rampart is the only

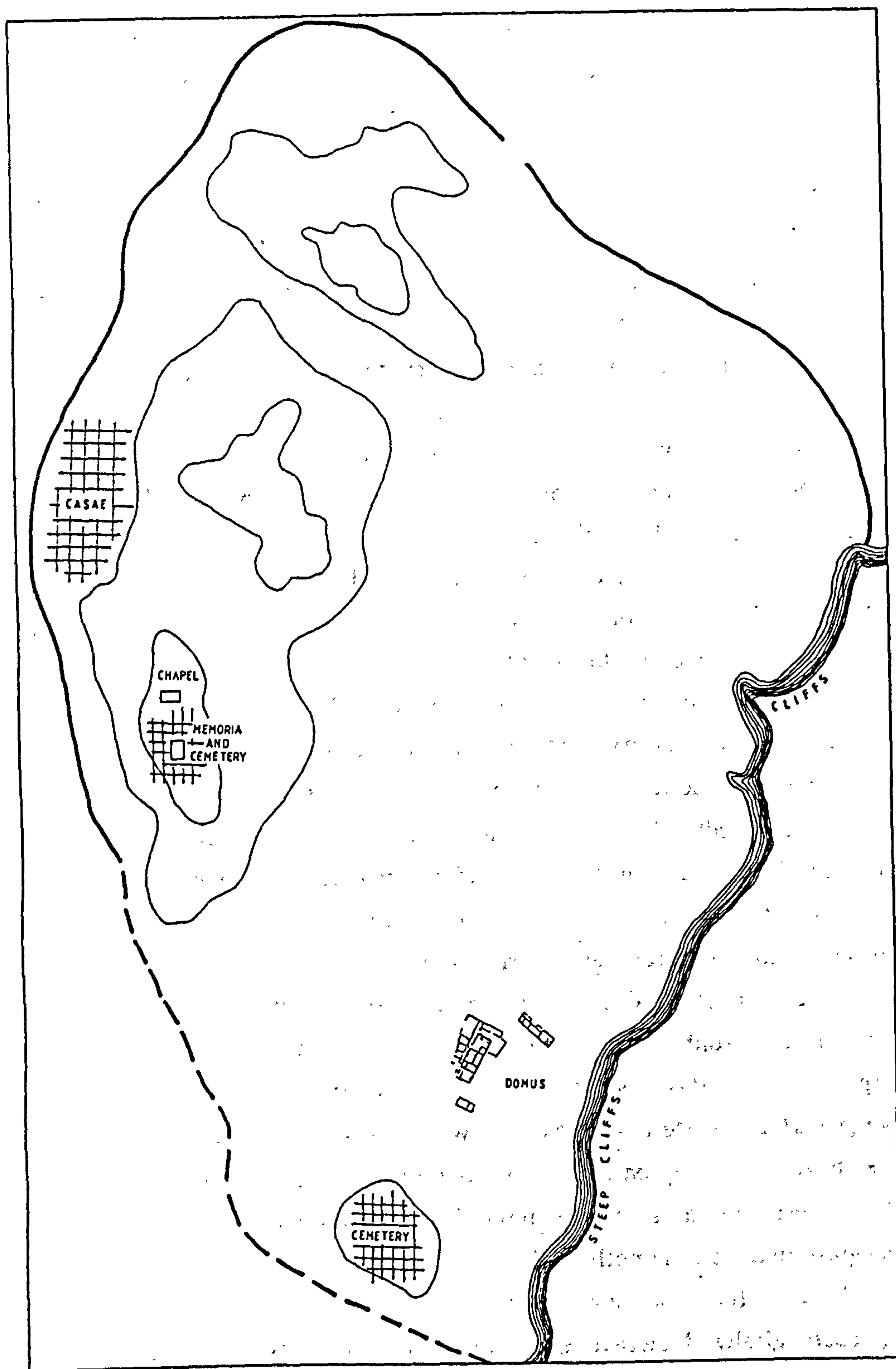


Fig. 4.7 Camp de Larina (1:1000) (redrawn after Porte).

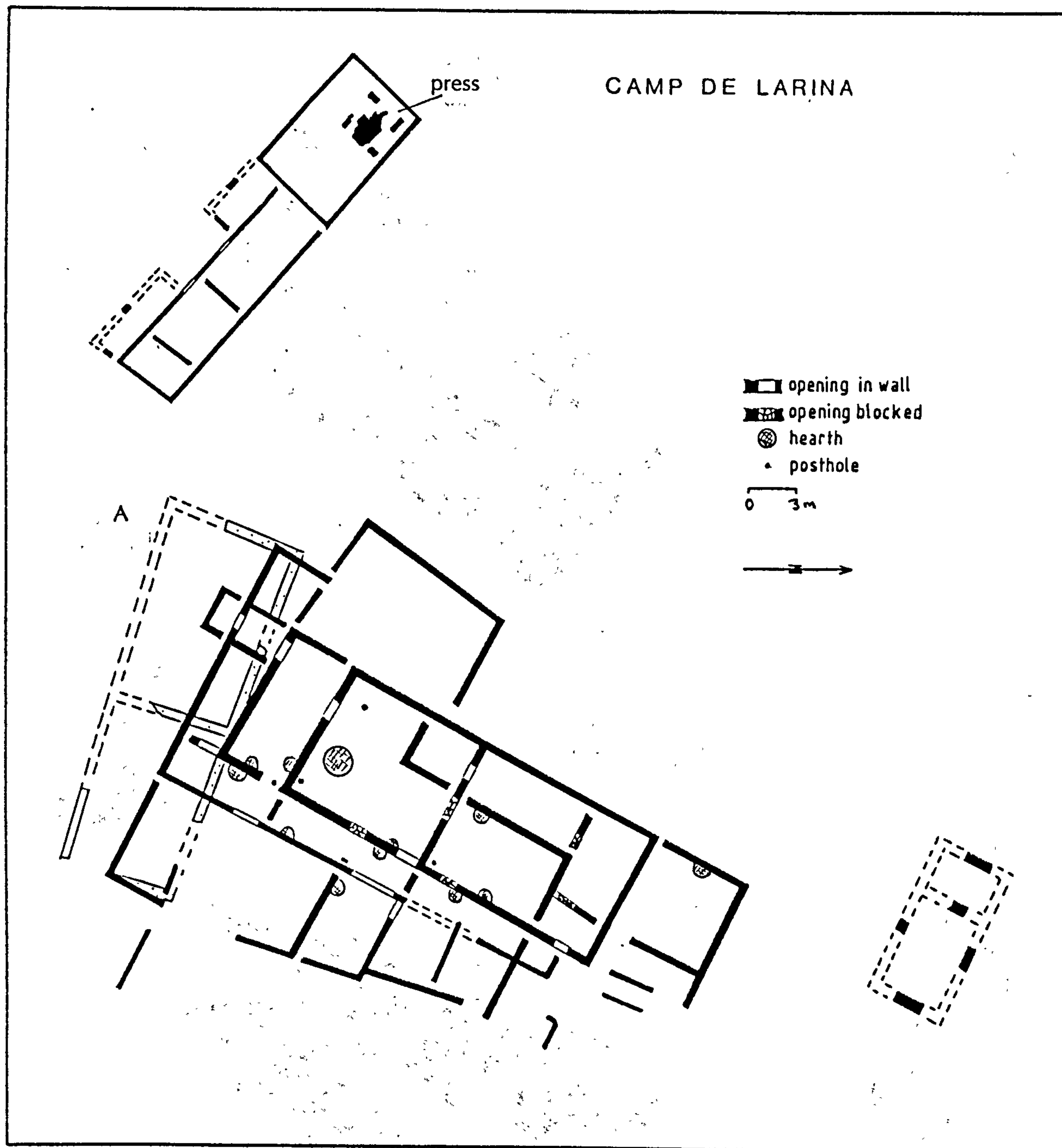


Fig. 4.8 Plan of the domus at the Camp de Larina, and the pressing room to the west (after Porte).

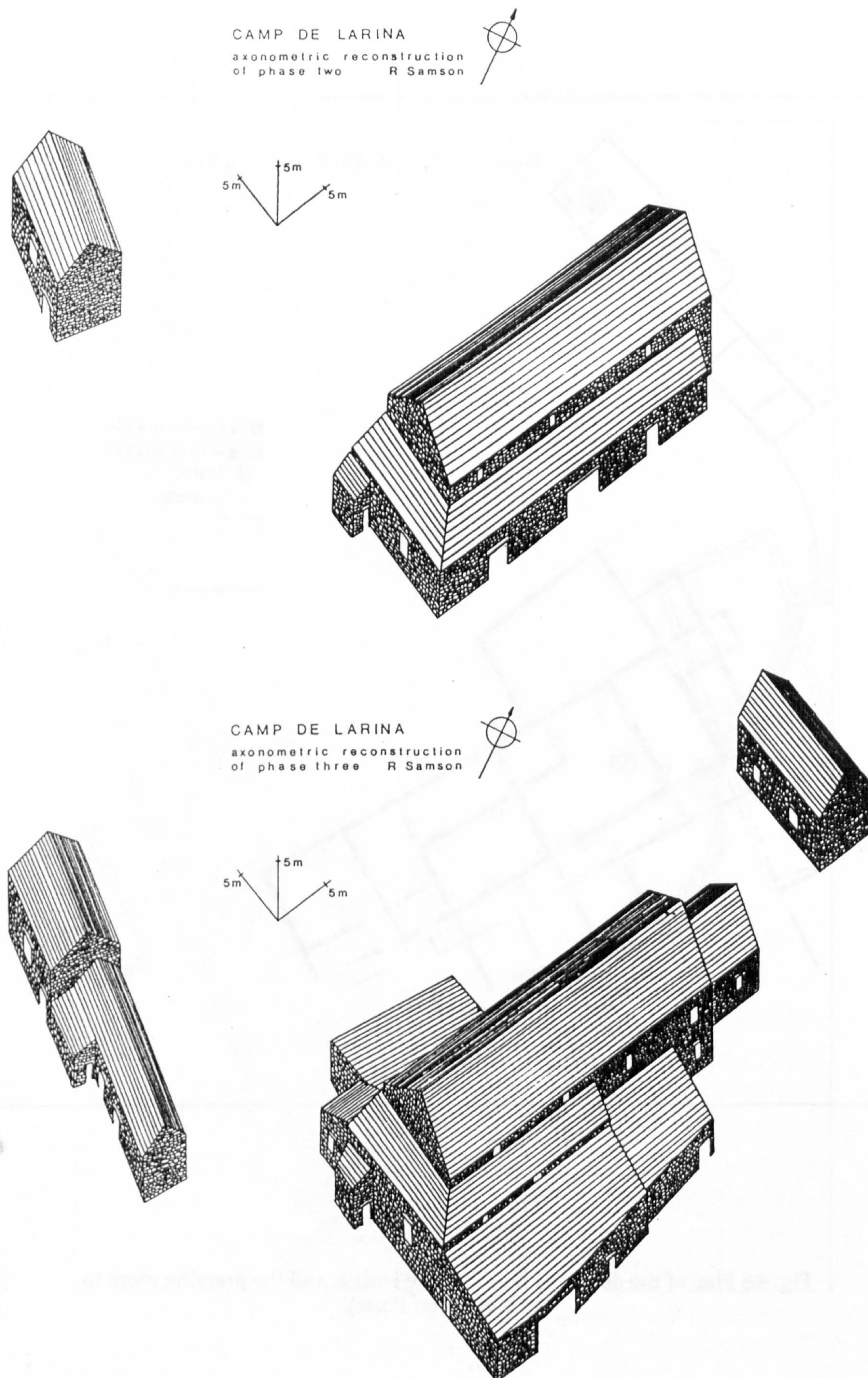


Fig. 4.9 Axonometric reconstruction of phases two and three of the Camp de Larina Merovingian villa buildings.

permanent water source on the site.

To the west of the so-called hamlet, at the summit of the hillock overlooking it, again uninvestigated, is a site thought to be the remains of a chapel and cemetery. Enough is apparently visible from the surface to suggest two small rectangular buildings and burials in slab-lined graves. At the northern extremity of the enclosure, a small burial ground has been excavated, again found on the summit of a small hillock.

Finally, just south of this second cemetery was found the main building complex, presently considered to be composed of three distinct buildings. The major building underwent a long and complex evolution (figs. 4.8, 4.9). At least eight phases are recognised, beginning with a two-roomed rectangular building 23 x 9 m. in dimension (A), quickly replaced by a similar building, 25 x 12 m., which overlay it on a completely new orientation. Further phases subdivided these rooms; small rooms were built on the eastern and western ends, and an L-shaped corridor was added around the south-west corner. Against this and the southern wall which had not been enclosed by the corridor-like addition, further rooms were added. It has been questioned whether by its final phase the earliest northern rooms remained in use; certainly a number of internal doorways were blocked up at this point.

To the north of the main building were found vestiges of a building running east-west, the opposite orientation to that of the main building. Little investigation of this area has been made, or at least published. To the south a long rectangular building of several phases has been uncovered. At the end of the western-most room was found a large square pit cut into the rock floor. Four square recesses were cut into the rock around the central hole, one of which still contained a large snugly fitting block, presumed to have come from the Roman temple. The room has been interpreted as having been used for pressing, presumably grapes.

Bibliography: Porte 1980; 1984.

Vilauba (Catalunya, Spain). From the limited area of excavation it was felt that this had been the site of a Roman villa whose origins were probably in the Republican period, although the first major construction to be apparent in phase II is much later; the latest alteration of this phase was dated to after the mid-fourth century. Phase III saw many alterations, centred on a room probably adapted as a *torcularium*, 'pressing-room', dated to the fifth century. Phase IV involved a more radical rearrangement, which included increased organisation, higher building quality, and increased scale; there is no doubt

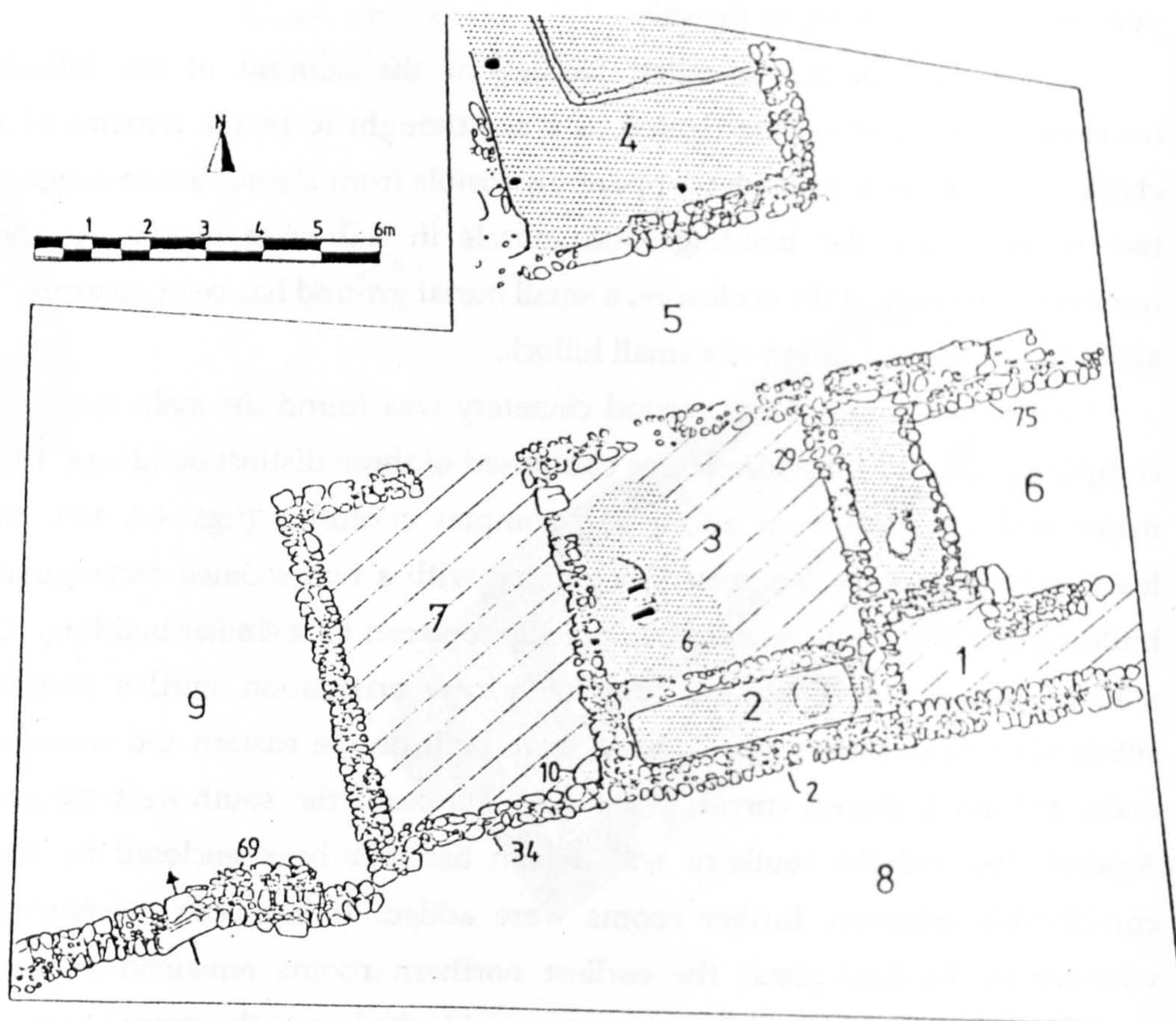


Fig. 4.10 Vilauba, the pressing room of a Visigothic villa, phase IV (after Jones et al.)

about the existence of a pressing-room in this later phase (fig. 4.10). Sherds of paleochristian grey ware, a late Roman C bowl, African Red Slip, and late Roman amphorae, date phase IV to the sixth century. Demolition involved the careful retrieval of large chunks of flooring and stone from walls. The date when occupation ceased is argued to be no earlier than the seventh century. The most striking feature of the site is that Roman building traditions continued, and in this portion of the villa with its floors of mortar and *opus signinum* and mortar-bound stone walls, even increased in quality during Visigothic times.

Bibliography: Jones *et al.* 1982.

Reccopolis (Guadalajara, Spain). *Reccopolis*, sixty kilometers east of Madrid, is perched on a plateau cut off from the rest of the high plain except by a small land bridge at the eastern end (fig. 4.11). The Tajo river flows along the northern side and has cut a 55 metre cliff. Around the remaining sides the plateau is some 30 metres above the valley. The plateau top is divided into an upper and lower 'town'. An enclosure wall probably ran along the entire

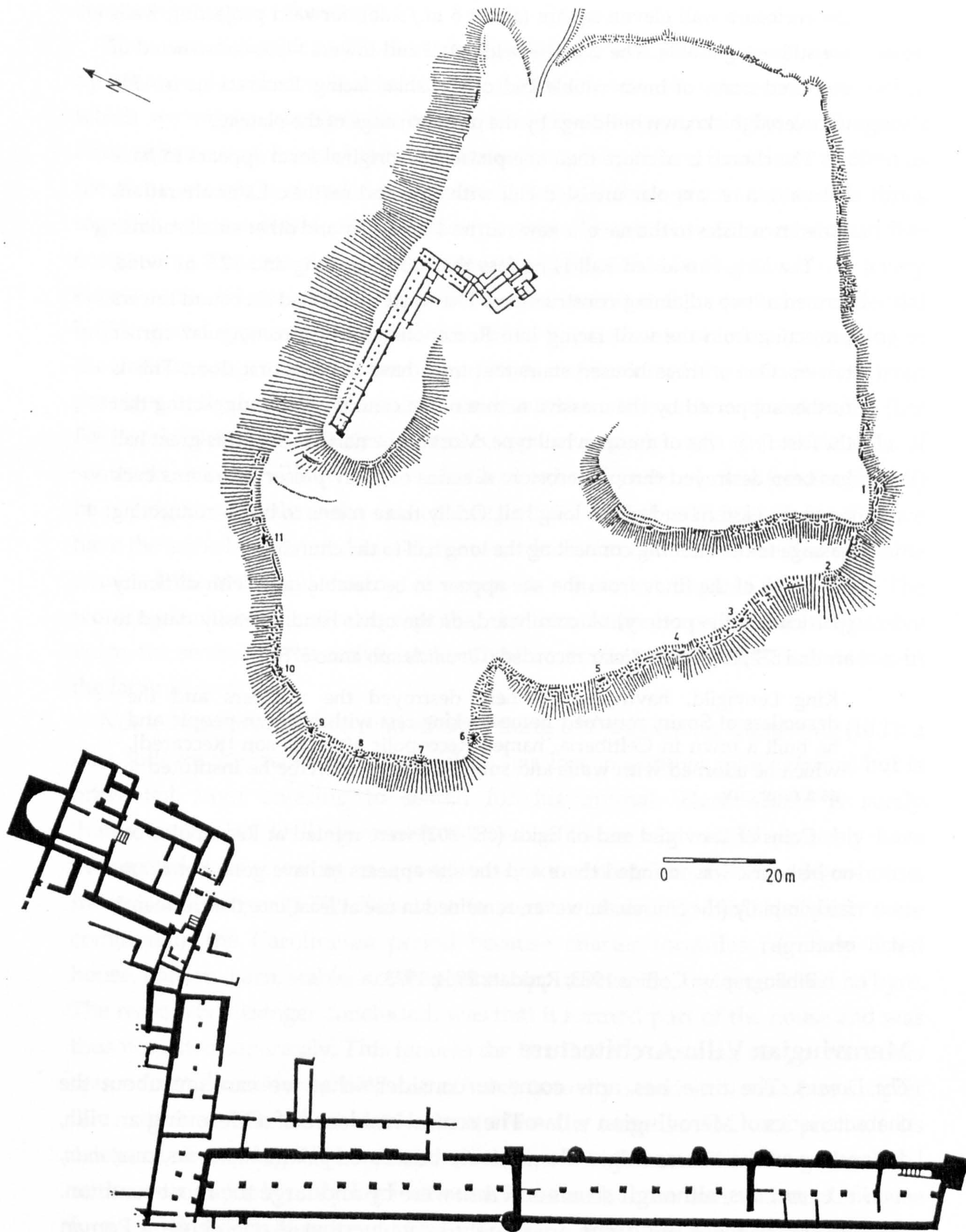


Fig. 4.11 Reccopolis, a Visigothic royal palace-town (after Raddatz).

perimetre (although the stretch along the northern face has been eroded away), thus attaining a length of 1.6 kilometers. Around the western half of the enclosure wall eleven square (about 6 m./side), forward projecting walls are still recognisable. The 2 metre wide wall and towers were constructed of mortared stone, of inner rubble and outer ashlar facing. Excavations in 1945 uncovered the known buildings by the northern edge of the plateau.

The church is of more than one phase. The original form appears to have been a thin rectangular unaisled hall with apse and narthex. Later alterations added two aisles to the nave, a new narthex, transepts, and other small rooms.

The long two-aisled hall is no less than 132.5 m. long and 12.5 m. wide, formed of two adjoining constructions. The western half had six round towers projecting from the wall facing into Reccopolis and two rectangular corner towers. One of these housed stairs that must have led to a first floor. This is further supported by the massive nature of the central pillars, suggesting that the first floor was of an open hall type. Most of the north face of this great hall has been destroyed through erosion. A series of badly preserved rooms back on to the eastern end of the long hall. Oddly there seems to be no connecting passage to the building connecting the long hall to the church.

Many of the finds from the site appear to be datable only with difficulty (particularly the pottery). A coin hoard, on the other hand, is easily dated to around 577/8. John of Biclar recorded (*Chronicle* sub anno 578):

King Leovigild, having everywhere destroyed the usurpers and the despoilers of Spain, returned home seeking rest with his own people and he built a town in Celtiberia, named Reccopolis after his son [Reccared], which he adorned with walls and suburbs and by a decree he instituted it as a new city.

Coins of Leovigild and of Egica (687-702) were minted at Reccopolis, but no bishopric was founded there and the site appears to have gone out of use fairly rapidly (the church, however, remained in use at least into the thirteenth century).

Bibliography: Collins 1983; Raddatz 1964; 1973.

Merovingian Villa Architecture

The Domus. The time has now come to consider what we can say about the characteristics of Merovingian villas. The central residence of a Merovingian villa, depending on the source might alternatively be termed *palatium*, *domus*, *casa*, *aula*, *mansio*, or *metatus*, although *domus* and *casa* were by and large the most common. Descriptions of these buildings are largely so vague that if one chose a Roman villa as a preconceived model, the descriptions would not disappoint, or if one

picked the image of timber halls in a Warendorf village fashion, as Dolling (1958) did and as do Chapelot and Fossier even more explicitly, the evidence would completely support the view.

There are difficulties with the term *domus* for it was used in a very wide sense by Merovingian clerics. It is interesting that the terms *cellula*, *cubicula*, and *salina*, which we might translate as 'room', were used very sparingly. In Gregory's *Historia francorum*, he used the term *domus* over sixty times, four times as often as the terms *mansio* and *metatus* combined, thus all three over seventy-five times together, while *cubicula*, *cellula*, and *in pensilem* do not account for more than five occurrences. To some extent the answer would appear to be that *domus* was a very elastic term. 'Home' could expand to take in more than just the residential building and apparently could shrink to the approximate of 'room', according to the various usages I have been able to find. Expansion of the term is clear from passages in the barbarian laws such as *domus sive curtis* or *in curte aut in casa* (*Lex Salica* 6.3; 34.4) in which *domus* is used synonymously with the whole villa. If someone attacks a villa (*villam . . . adsalliert*) or breaks into a house (*casam effregerit*) he is judged culpable at the rate of thirty *solidi* (*Lex Salica* 17.1; 11.3); in effect we have the same law restated in different words. In *Lex Gundobada* we find the same crime of the wrongful cutting of a woman's hair in chapters 33 and 92. The wording is almost identical except that the first clause is recorded in the passive voice, the second in the active, and in the former the crime occurs *in domo sua*, in the latter *in curte sua*.

Not infrequently do we read of something like that in *Lex Gundobada* (16.1): a man follows the tracks of an animal he has lost to another man's *domus*, but is prevented from entering to search for his animal. Here *domus* is surely 'homestead'. Had Dolling considered such a clause, she would probably have interpreted it as referring to a longhouse, something like a Frisian byre-house. Indeed, Schlesinger (1979, 596) came to the conclusion that *Wohnstallhäuser* were common in the Carolingian period because charter formulas regularly listed house, garden, barn, stable, and hayloft as appurtenances, but mentioned no byre. The reason, Schlesinger concluded, was that it formed part of the house and was thus not listed separately. This ignores the fact that the wording of appurtenances was a formula, not a detailed list of what actually stood in a villa (see chapter two, 'manses'). The absence of a byre from the formula does not preclude its existence. Tempting although the parallel of Feddersen Wierde and related settlements might be, they lay well outside the area of the laws and similar house forms at this date have yet to be found away from the predominantly coastal distribution along the north German plain (Trier 1969). Moreover, Feddersen

Wierde is perhaps a century too early, and although byre-houses are found throughout the Middle Ages and beyond (following a gap in our archaeological knowledge in the sixth and seventh centuries) we find that the Feddersen Wierde style byre-houses were largely replaced along the north sea coast by separate dwellings and byres by the Carolingian period. Until now no positive evidence for such byre-houses south of the North Sea coast has come to light (they are absent from Anglo-Saxon settlements too), and it seems better to interpret the various regulations against driving 'home' lost or intrusive animals to mean nothing more specific than that.

The extended meaning of *domus* works in the opposite direction as well and may have been used to mean simply 'room'; or perhaps a one-roomed building is meant. Gregory writes, perhaps with disapproval, that after being called by King Chilperic to the royal villa of Berny-Rivière all the bishops had to reside in *unam domum* (HF 5.49). Why should he mention it? If indeed he did disapprove, was it because a single *domus* could not offer the bishops private quarters? Even more positive evidence to suggest that *casa* and *domus* could be used to mean a room or a single-roomed building comes from the use of the expression *angulus*. Gregory (HF 10.27) relates an anecdote in which two men were killed in the corner (*angulus domus*) of a 'house', or room. In the *Lex Salica* (58.1) one accused of killing another can clear himself by an oath, in the course of which he must go to his house (*casa*) and collect a handful of earth from each of the four corners (*de quattuor angulos terram in pugno collegere*). Such an expression conjures images of a timber hall with a beaten earth floor.

Thus we are led to consider now the material of house construction. Timber clearly played an important role, but stone almost certainly played a major role in buildings of some pretension. Now it may be that Dolling and Chapelot and Fossier are correct in thinking that the *domus* or *casae* of the barbarian laws were like the timbered halls of Warendorf or those excavated in Saxon England. Dolling (1958, *passim*) might have been right to conclude that the *domus* has a special status; that only freemen or freedmen owned a *casa*, while slaves and serfs are seldom recorded as occupying them. Their dwellings were more often than not called huts (*turgium*). However, freemen include a much larger portion of the population than the highest ranking, whose residences are the subject of this thesis. It is quite plausible, therefore, that the difference between the *domus* Dolling imagines from the *leges* and the *domus* painted by Fortunatus is that of the domiciles of the common freemen and those of the great men.

Returning then to the material used in the construction of the great villas, we saw above a poem by Fortunatus about a wooden *domus*. A wooden palace he

called it. No need for all that sand, chalk, clay, and stone, for wood could take the place of all these various materials. Although not otherwise known for being satirical, there is certainly a possibility that Fortunatus was here employing satire. In his other poems neither stone nor wood is expressly mentioned, although Mediolanum had marble columns according to Fortunatus. Perhaps because his poems fit into a late classical mould or perhaps because of the frequency of the appearance of baths and porticoes, it is hard to imagine these villas as not being largely built of stone.

Gregory of Tours (HF 4.46) relates that when Andarchius was burnt to death in Ursus' *domus* by Ursus' servants, they piled combustibles against the doors which were made of wooden planks (*ostia domus, quae erant ex ligneis fabricata tabulis*). The implication is almost that the walls were not made of wood (although it could just be that the doors were specifically ignited first to prevent easy escape). At the Camp de Larina, the buildings, including the work building housing the grape press, were all of stone. There is nothing to suggest that this was a site of extraordinary pretensions so that it would seem admissible to extrapolate from this that many other villas were built of stone. The palaces of Gothic neighbours, at Ravenna and Reccopolis, were certainly of stone. At Reccopolis and Vilauba we also find mortar being used and cement floors.

One other element of building construction which would put Merovingian villas in a classical tradition is the probability that houses were roofed with tiles. At Berny-Rivière Gregory conversed with Bishop Salvius, who saw the naked sword of the wrath of God hanging over King Chilperic's *domus*. Gregory could only see the new tiling (*supertegulum*) the king had recently had put on the building (HF 5.50); the site of Reccopolis is littered with roofing tiles and the PALATIUM mosaic of Theodoric's palace at Ravenna shows it clearly tiled.

Of the *domus* itself, Fortunatus gives us little information. Clearly it was well-built, impressive, or beautiful, in order to be praised, but Fortunatus also describes one *domus* as nearly a *castellum* and one *casa* as 'powerful'. The hint of fortification has not escaped many, although the idea of a fortified *domus*, I argue, must be discarded.

Gregory and the barbarian laws both give the impression that most *domus* were unfortified. This is predominantly the result of the circumstances in which most *domus* are described, namely as the setting for murder, rape, arson, or theft. Thus, Sichar with a gang of armed men broke into the *domus* of Auno and killed him, his son, brother, and servants (HF 7.47). Chuppa assembled some of his men, broke into the *domus* at Mareil and attempted to carry off the daughter of the late Bishop of Le Mans. Her mother assembled her servants and drove Chuppa off,

killing some of his men (HF 10.5). Ursio and Berthefried broke into the *domus* of Lupus (HF 6.4). Berulf's *domus* was ransacked by slaves (HF 8.26). *Lex Salica* (17.1) sets the fine at thirty *solidi* if anyone attacks another's villa (*si quis villam alienam adsallierit, mallobergo alafalcio*), even generating a legal term which would apparently describe such a crime; *alafalcio*. Attacking a villa would appear to be comparable to breaking into a house (*casam effregerit*) and stealing two *denarii* worth of goods (*Lex Salica* 11.3). Such penalties and prohibitions are repeated in other laws (e.g. a law forbidding that *domum violenter ingressus fuerit* in *Lex Ribuaria* 49.3; *Lex Gundobada* 15.2; *Lex Iudicorum* 8.2.1).

When Duke Beppolen forcibly entered the *domus* of the citizens of Angers, he did not wait for the keys, but broke down the doors (HF 8.42). Indeed, the security of a Merovingian house seemed to rely on no more than the simple strength of the lock on the door. On hearing of Chilperic's death, Duke Desiderius seized the treasure of the princess, Rigunth, in Toulouse, placed it in a building, sealed the doors, and left a strong force of men on guard (*in domo quadam sub sigillorum munitione ac virorum forcium custodiam mancipat*) (HF 7.9). Gregory (HF 10.2) describes a bloodbath which occurred in Carthage involving locals and the Frankish embassy. The battle occurred at the door to the Franks' lodgings, resulting in so many dead bodies that Grippo had to step over them to get in. *Lex Salica* (11.5) expressly forbids illegal entry gained by breaking the lock or using a false key (*si vero clavem effrigerit aut adulteraverit, sic in domu ingressus fuerit*). Finally we can say of our one excavated Merovingian *domus*, at the camp de Larina, that it was certainly not fortified, and in its final phase its multiplicity of doors would have made determined effort at entrance much easier. The question of a fortified enclosure around the *domus* is another question to which we will return in a moment.

Of the composition of various rooms within a *domus* little can be said. Beds in which to sleep, die, or fornicate make regular appearances, not only in Gregory's anecdotes, but also in the laws. There also appears to be good circumstantial evidence that beds were to be found in separate rooms. Andarchius would retire (*se collocat*) in his *domus* to sleep, implying perhaps a bedroom or at least a far corner of the room (HF 4.46). The lover of Ambrosius's wife entered the *domus* and killed Ambrosius and his brother in the same bed without waking the household (HF 6.13). Because the household failed to be wakened despite the fact that Ambrosius's brother Lupus screamed repeatedly for help, we can infer that they were in a bedroom, which was either far from the remaining rooms, had very thick walls, or was itself a separate building. Desideratus, intent on killing Syrivald, approached the villa called Fleury-sur-Ouche with some of his men,

killed one of Syrivald's friends as he came out of the *domus*, and eventually came to the room (*cellulam*) where Syrivald was accustomed to sleep and found the door could not be forced. He had to knock down a side wall (*uno latere parietem*) before he could kill him (HF 3.35). Rauching was killed in *cubiculum intromiti* and was thrown out of a window (HF 9.9). Lothar II's queen Bertetrudis was frightened by Leudemund at Marlenheim and withdrew in *cubiculum* (Fred. 43). Etymologically these *cubiculi* should be bedrooms, and we do find a bishop asleep in a *cubiculum* on one occasion (HF 6.36). To this there is little more to add, but it is suggestive that a well-to-do Frankish villa residence had more than one room and at least a separate bedchamber.

Werpin was killed in *pensilem domus*, whatever this means (HF 8.18). Thorpe translates it as 'toilet', M. Weidemann as 'heated room', and Loyn and Percival as 'women's quarters' which they extend from what they see as literally 'heated room'. From classical Latin *pensilis*, 'hanging', the word originally had the meaning of 'suspended on columns/arches' as an architectural term. Thorpe perhaps translates the term as 'toilet' because of the term's slang meaning for the male sexual organ; this etymology can surely be rejected. 'Heated room' is the more plausible translation, its meaning derived from its under-floor hypocaust: a room built on columns. It is tempting to see this as evidence for a Roman villa still in use, or the continuity of the traditional Roman fashion of heating homes.

Textual evidence sheds no further light on the functions of individual rooms, but has much more to say about different buildings. Dolling (1958, *passim*) reveals that women's quarters, barns, granaries, byres, and pens for pigs are all mentioned in the various barbarian laws. She tabulated the terminology from the various laws for the different buildings. This has been subsequently retabulated by Chapelot and Fossier (1985), who append both the ground plans and reconstructions of various timber buildings from the excavation of Wareendorf 'village' in such a way that words and plans correspond (fig. 4.12).

The archaeological evidence from the Camp de Larina reveals that the *domus* contained a large number of rooms, many of them quite small, and a corridor. Although we cannot say much about the function of the various rooms, we can note that the arrangement is more reminiscent of Roman villas than Germanic timber halls. Yet the pressing room and the other stone built agricultural building by the *domus* at the Camp de Larina are not connected to the residential block. In other words, unlike traditional Roman villas there is a tendency towards fragmentation into individual buildings of separate function. This was something noted in the last phase of the late Roman villa at Echternach too (chapter three). This probably explains why *domus* was used by Gregory of Tours and his

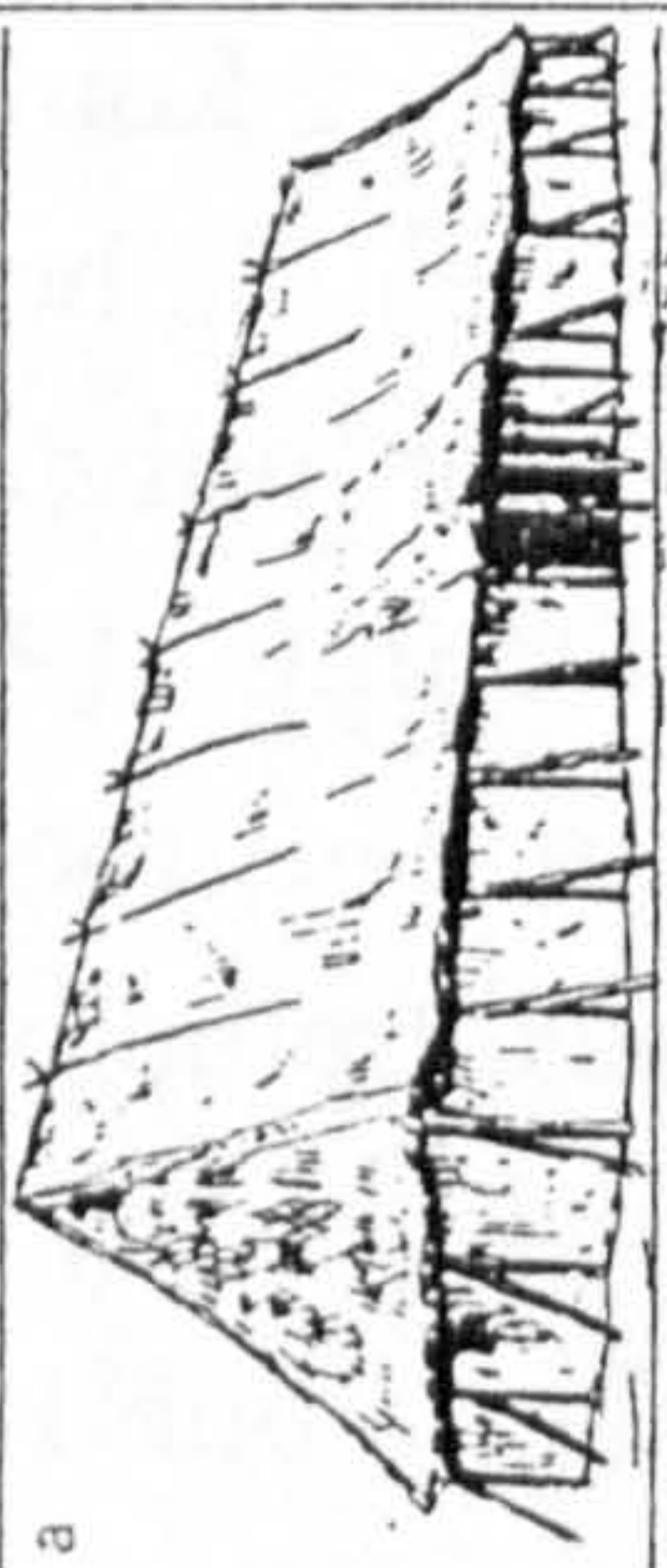

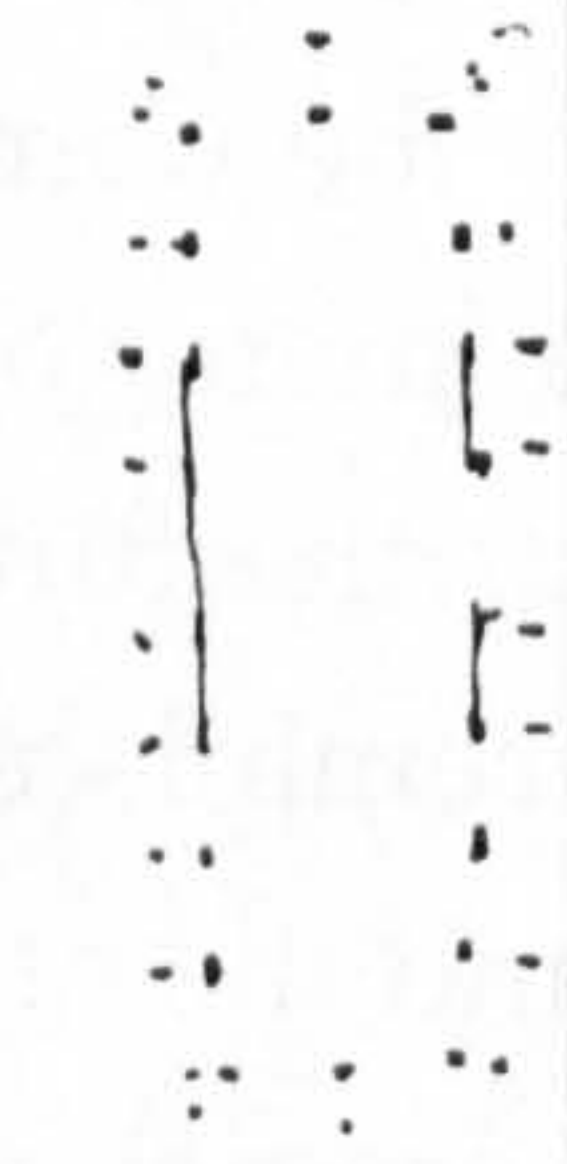
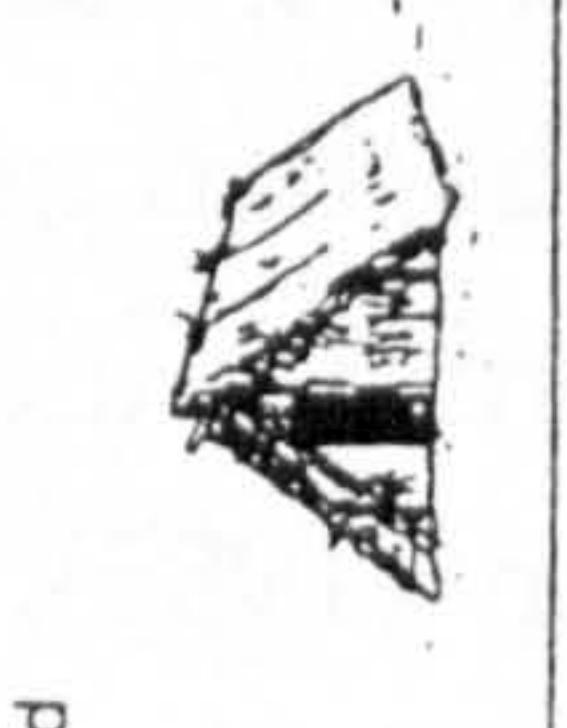
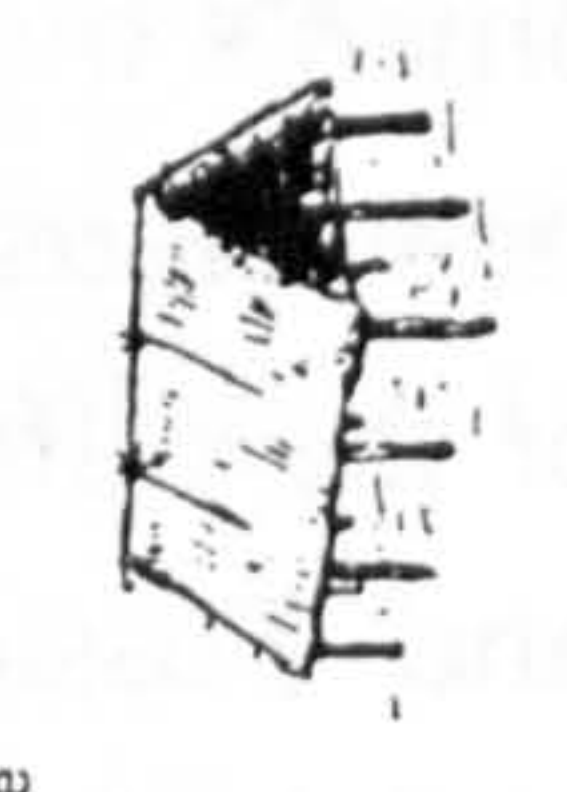
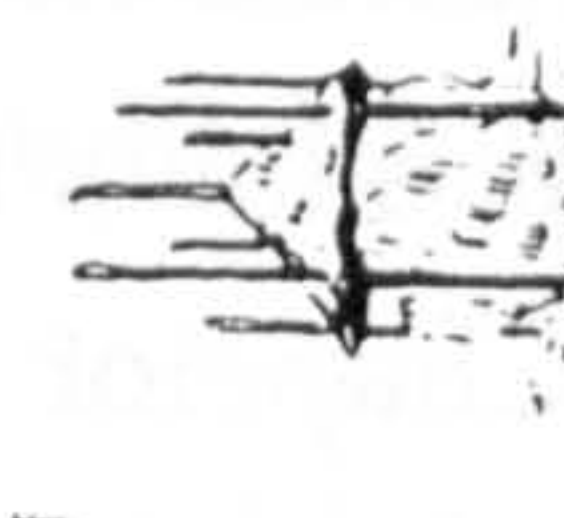
| Warendorf | | Lawcodes | | | | | | |
|----------------|---|--|---|--|----------------------------|-------------------------|---|--|
| Ground – Plans | Reconstructions | Allemans | Bavarians | Salian Franks | Ripuarian Franks | Saxons | Anglo-Saxons | |
| a |  | - domus lib. - casa - sala | - domus lib. - casa dom. - seli, casa, hus | - domus lib. - casa - salina | - domus - hus - casa | - domus - casa | - hus - aern - healle - seli | |
| b |  | - scuria lib. u.a. - armenta equarum et vaccarum | - scuria lib. conclusa parietibus et pessulis cum clave munita | - scuria cum animalibus | | | - scipena - bern (horreum) | |
| c |  | - granicia - cellaria - domus servi | | - sutes cum porcis - cellarium | - sotes paricus | - stabulum - horreum | - styllan = stall-bauen - fald – lochyrdl = Hürdebauen | |
| d |  | - genicia - stuba spicaria - servi, ovile - pocaricia | - coquina - balnearius - pistoria - mita - mita minor - ria = scoparia | - genicium - screona | - ovile - appearius | - screona (alvearium) | - cyline | |
| e |  | - spicaria dom. - scuria servi | - scuria absque parietibus = scol | - spicarium aut horreum cum tecto | | - spyker | | |
| f-g |  | | - granarium = parc - fenile | - machalum = horreum sine tecto fenile | | - granarium | - hreac = machalum | |

Fig. 4.12 An attempt by Chapelot and Fossier (1985) to weld archaeological information from Warendorf to the named building types found in lawcodes originally summarised and tabulated by Dolling (1958).

contemporaries in a way which fluctuated between room, building, and villa: villas were fragmenting into dozens of separate buildings, many of them single roomed.

Nucleation. Percival suggested, as we saw in the last chapter, that the traditional Roman villa of the late Empire changed its morphology in the fifth and subsequent centuries, thus making its detection more difficult. One possible development he proposed was that of nucleation. Instead of half a dozen or more villas dotted on the landscape, perhaps the population represented by these settlements came together into something more like a village. In chapter two it was made clear that, in terms of population, villas were clearly smaller than *vici* which in turn were smaller than cities. But that they attracted new settlers and were capable of sustaining great population growth is clear from the fact that many of the Merovingian and Carolingian royal villas (and more particularly villas which were to become monastic centres) became large centres and indeed urban centres by the later Carolingian period. I have treated Reccopolis in this chapter as if it were a villa rather than a town, a decision made not only because the settlement was fairly small and never received a bishop, but because all the inhabitants were presumably royal dependants, whether slaves, serfs, dependent artisans, or retainers.

Mediolanum was clearly depicted by Fortunatus as having been populous, but Mediolanum was perhaps anomalous. Fortunatus calls it a *castrum*, clearly because of its walls and thirty towers. Gregory called Dijon a *castrum* and described its thirty towers as well, although it was clearly a late Roman town. In chapter two it was argued that the term was often applied to Roman *vici* which had been endowed with walls, and in light of the archaeological evidence from Niederemmel which points to a Roman *vicus*, it may be that Mediolanum is 'wrongly' classified here as a villa.

There are only a few hints in the writings of Gregory of Tours that nucleation had begun. As an ill omen Gregory records (HF 9.5) the report of a villa which vanished with all its houses and inhabitants (*villa cum casis et hominibus*). We also hear (HF 8.15) of the *populum villarum* led off in captivity and the multitudes of neighbours of the villas (*multitudo vicinarum villarum*) who fled to Vulfolaic. In the tale of the discord between Sichar and Chramnesind which Gregory tells, Sichar fled to his villa, but after beating his slave with a stick, the slave drew Sichar's sword and killed him. Chramnesind heard of this, hurried to Sichar's *domus*, where he killed some slaves, stole everything he could find, and burnt down not only Sichar's *domus* but those of others who lived in the same villa (*domus . . .*

quam reliquorum, qui participes huius ville erant) (HF 7.47). The implication appears to be that the houses were close enough together for the fire to pass from one to another.

Dolling's conclusions from the barbarian leges are similar. In a villa or *curtis* lived lord, freemen, freedmen, and slaves together. In *Lex Salica* (3.1) when a murdered man was found in a villa a judge was to go to the place and blow a horn, to which call ought come the neighbours (*vicini*). As in Gregory's terminology, the inhabitants of a villa are called neighbours. One of the *Lex Salica*'s (45.1) most famous clauses describes the procedure by which someone might have been prevented from moving from one villa to another (*super alterum in villa migrare*) should any single one of the *vicini* object. This implied to Dolling (1958) that a villa could on occasion be considered a village.

Such semantic worries may obscure the situation more than clarify it. Percival (1988, 10) questions whether Agache makes a valid distinction between modern nucleated settlements and the dispersed Roman villas, whether farm is an apt description of a courtyard villa 200 metres in length:

These villas, with quite large concentrations of people in and around them, are surely little communities in their own right; in terms of population the difference between them and a modern village of, say, 200 people is a rather artificial one.

Whatever the value of calling Roman or Frankish villa settlements with their small populations villages, the evidence does seem to imply close proximity of the inhabitants one to another. Such would make the necessary agreement of neighbours to the advent of a newcomer more explicable; they would live in very close contact with one another. Dolling is quite right that the ultimate authority about who came in lay with the lord, for this was no autonomous primitive democratic or communist community as the Germanic school once sought to portray it, capable of making its own independent decisions. Certainly not when a punishment might be inflicted on freemen within the villa who negotiated (*negotiaverit*) with strangers without the lord's knowledge (*Lex Salica* 4.8) and not when a *conductor* in a villa had the power to assign accommodation to travellers who stopped for the night (*Lex Ribuarica* 38.10). Freemen and half-free, however, might actually possess their own *casa* rather than live in one provided by the lord (*Lex Salica* 50.1). Here is how a seventh-century villa might have looked as postulated by Percival (1976, 177):

a village of perhaps a few hundred people, grouped round a manor house, which would itself be in some form of fortified enclosure; the manor house would still retain much of its Roman structure, though this would be to some extent concealed by the later accretions and dilapidations.

To some extent this might be seen as describing the camp de Larina, although there, it must be stressed, the building which must surely be seen as the villa owner's *domus* is some distance from the nucleated 'settlement' which is interpreted as being the dwellings of the villa serfs. The physical separation of the lord's home from those of his servants is a hypothesis one might prefer to accept until proved otherwise.

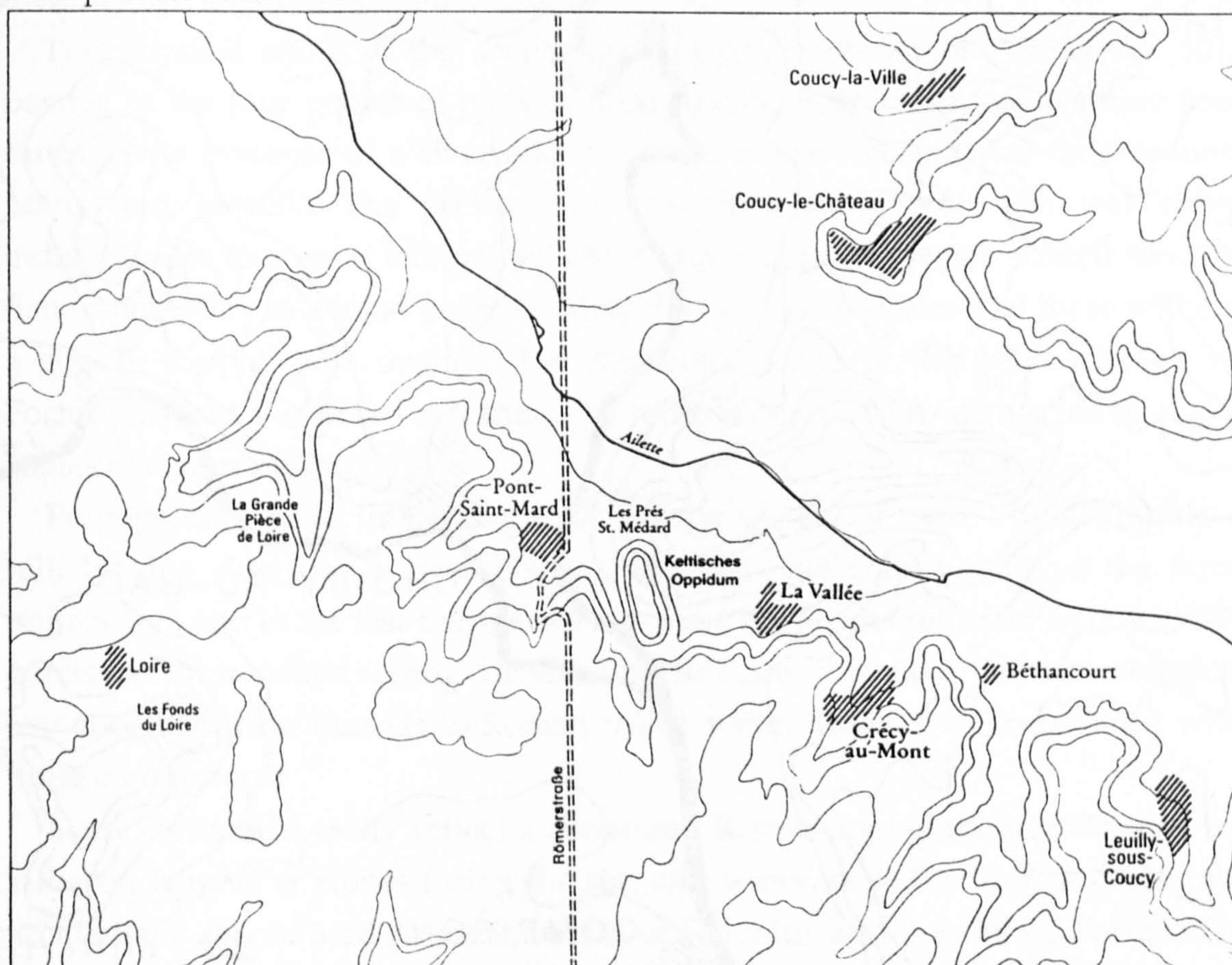


Fig. 4.13 Bishop Bertram of LeMans's estates held in the region of Soissons (ca. 1:65,000) (after M. Weidemann).

Topographic Setting. The location of the various royal Merovingian villas at which residence was attested (fig. 4.4) appears to have favoured proximity to Roman roads and major rivers. Along the Seine are Vaudreuil, les Andelys, Conflans, Chatou, Rueil, Épinay, and Clichy. Along the Oise: Creil, Pont St-Maxence, Verberie, Venette, Compiègne, Choisy-au-Bac, Mélicocq, Montmacq, and Quierzy. Compiègne in fact lies at the confluence of the Oise and the Aisne and further along the Aisne was the important royal villa of Berny-Rivière. Chelles, Nogent, Noisy-le-Grand, and Lagny all lie along the Marne, and Péronne, Éterpigny and Athies lie by the Somme. On the Paris-Rouen road were Les Andelys and Étrépagne; on the Paris-St-Quentin route were Luzarches, Creil, Pont St-Maxence, and Venette. From Meaux to Senlis there were Ver and Lagny-

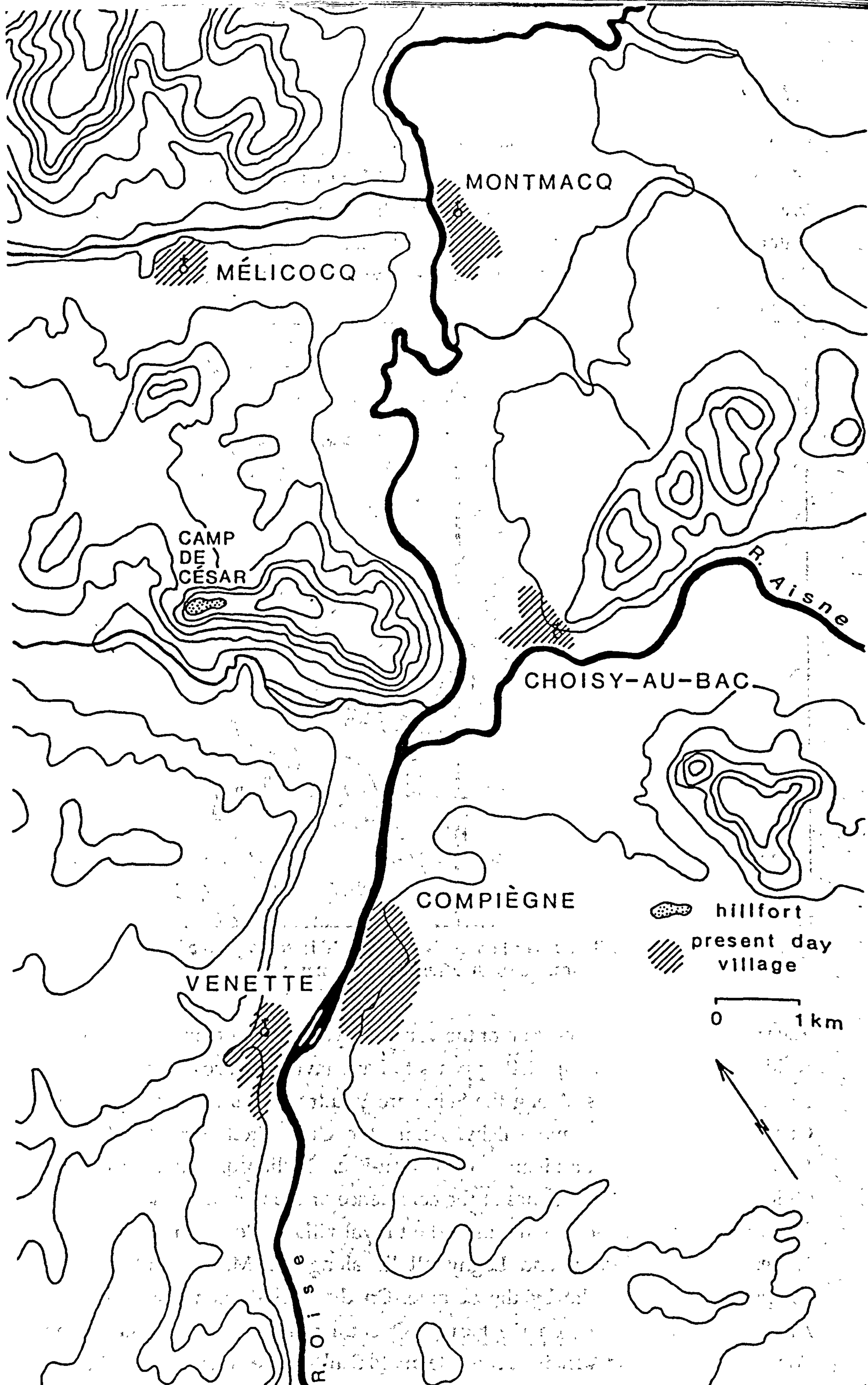


Fig. 4.14 Distribution of important royal Merovingian palaces at the Oise-Aisne confluence.

le-Sec, and between Soissons and Beauvais lay the villas Berny-Rivière and Compiègne. On the road from Soissons to Amiens are found Choisy-au-Bac and Venette, Ponthion lay on the Châlons to Toul road, and Marlenheim on a major road west out of Strasburg. The importance of access is very clear, and there is something quite Roman in the setting of these villas, the most important rural dwellings in the Merovingian kingdom.

The physical siting of the *domus* appears in almost all of Fortunatus' villa poems. In the four poems of named villas quoted in this chapter, we hear four times of the presence of a river and usually a major river at that; the Garonne, Loire, and Moselle. The presence of a river teeming with fish was either indispensable to a great landowner's favourite estates, or poetic conceit thought fish so necessary to a good poem lauding the praises of estates that those without a goodly supply were unsung. The particular siting of the villas extolled by Fortunatus had the added advantage of lying along rivers which acted as major routes of communication.

Perhaps even more interesting is the image we get of these villas lying up a hill, looking down to a river below. Sidonius Apollinaris gives us the same impression, and in the last chapter we saw how his poems reflected a consciously perceived appropriate setting for villas by Romans, and how the archaeological evidence confirms that Gallo-Roman villas were situated in accordance with those conceptions.

Does Fortunatus really reflect a continued Roman topographic preference for villas on heights or slopes facing the sun and water, or is the Roman preference accidentally reproduced in his imitation of Sidonius and Ausonius? Fortunately we have archaeological (or better geographic) means at our disposal to check this. Because the names of Merovingian villas survive to this day in modern settlements, we can locate them and investigate their preferred topographic setting. A quick glance at the properties of Bishop Bétram of LeMans north of Soissons shows clearly the preferred setting on the slopes overlooking a river valley (fig. 4.13). Note too that the hillfort site of Les Prés St-Médard is avoided. This is likewise to be seen on the 1:100,000 map of royal Merovingian villas just immediately to the west, at the Oise-Aisne confluence; the villas lie very close to the major rivers and far from the hilltop settings suited to hillforts, thus avoiding the Camp de César (fig. 4.14). But most importantly, the map of Bishop Bétram's possessions in the Soissons region reveal the villas Pont Saint-Mard, La Valle, Crécy-au-Mont, Bethancourt, and Leuilly-sous-Coucy all lying along a similar contour line and between 1000 and 2000 metres apart; these are precisely the sort of figures Agache (1978, 352) notes for Roman villas in Picardie (chapter three).

Bishop Bertram's will lists villas and other rural settlements, but is not precise about their importance or size. Thus we cannot be certain that some of the sites were not small daughter settlements, effectively off-shoots of estates. Furthermore, the villas themselves might well be small farmsteads without any manor-like centre. For this reason I began this chapter by looking not at royal properties, but at royal villas at which residence is attested. Staying with this narrowed field of subjects for study, I have reproduced maps, using I.G.N. 1:50,000 maps, of a selection of villas from table 4.2 (with two early Carolingian royal villas). Those chosen were selected according to three criteria: that they were not spread over more than two maps (preferably on one); that the modern settlement was not so large that it obscured what I assume to be the original site (large towns and cities not only make the map difficult to redraw but also obscure features like streams and natural contours); and that there be as little doubt as possible about the stability of the settlement and the equation of the modern settlement with the named Merovingian villa. The following ten maps hopefully reveal something about the topography of the villas: Luzarches, Malay, Marlenheim, Ponthion, Pont-Ste-Maxence, Verberie, Ver-sur-Launette, Lagny-le-Sec, Brienne-le-Château, and Orville. It is assumed that the site of the parish church, or church with the oldest dedication is the best indicator of the former site of the Merovingian villa, and such churches are marked on the maps.

The maps confirm the inferences already made. Rivers, major and minor, figure prominently in all but, tellingly, Lagny-le-Sec, and Brienne which straddles a Roman road. The immediate area around each site offers settings well suited to hillforts that were not used by the villa: côtes d'Orleans by Luzarches, Chaumont by Malay, the Marlenberg or Wangenberg by Marlenheim and Kirchheim, the montagne de calipet by Pont-Ste-Maxence, la montagne by Verberie, or champ Simon by Brienne.

Villa Enclosures. One of the few questions ever raised about the architecture of Merovingian villas concerns the courtyard. In chapter one there was a quotation from Funck-Brentano in which the courtyard was claimed to have played a great part in the life of the community. It was perhaps too hasty to say that the image was drawn largely from his imagination, for it was drawn from the general conception of what a Carolingian villa was deemed to have looked like, to which we will turn in chapter six and seven. For the Merovingian period the evidence is but very slim. Suffice it to say here that the belief in the ubiquity of a courtyard in Carolingian villas stems in part from the document *Brevium Exempla* and in part from the etymology of the term *curtis*. The term was clearly derived from the

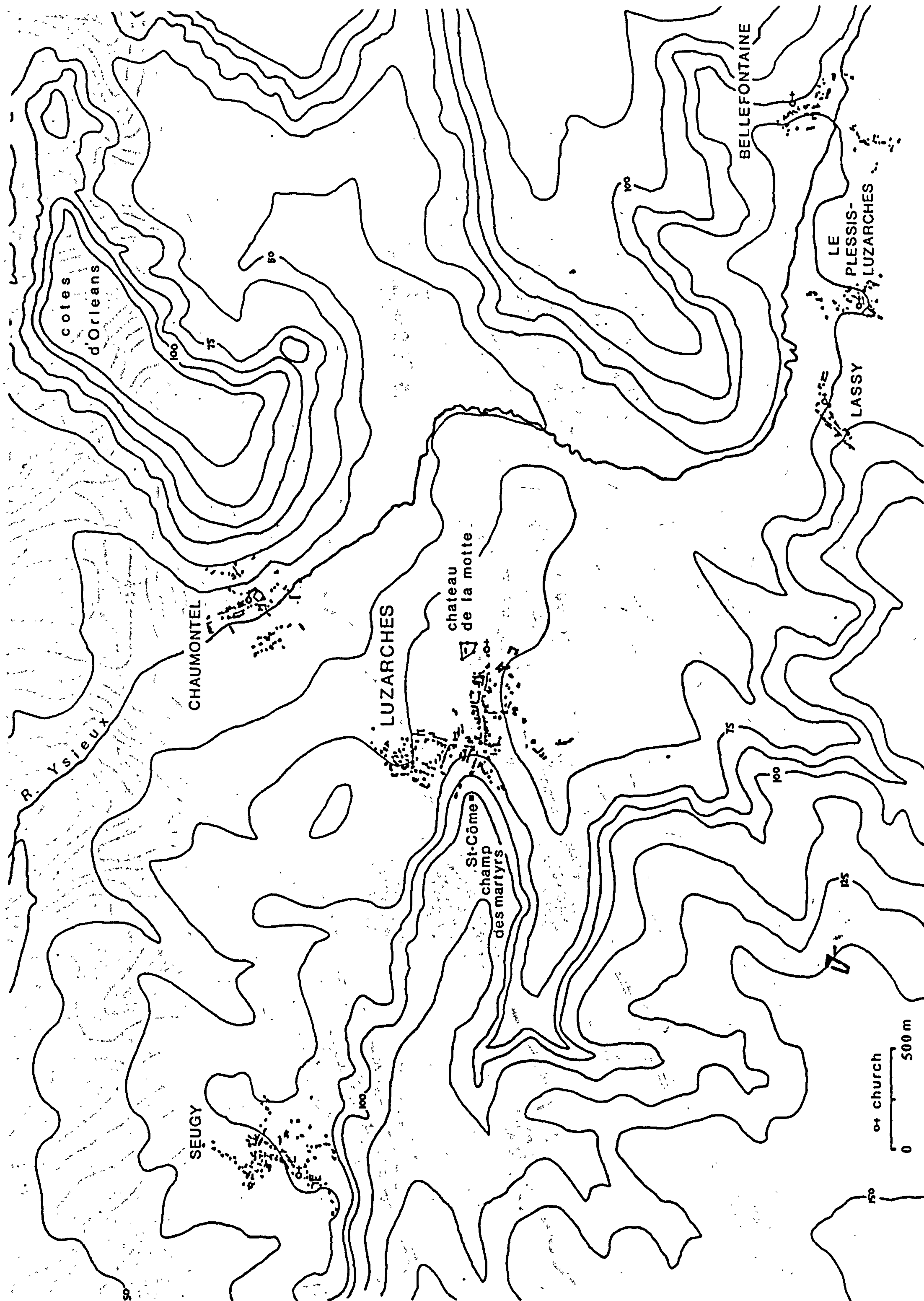


Fig. 4.15 Map of the are around Luzarches (from IGN 1:50,000).

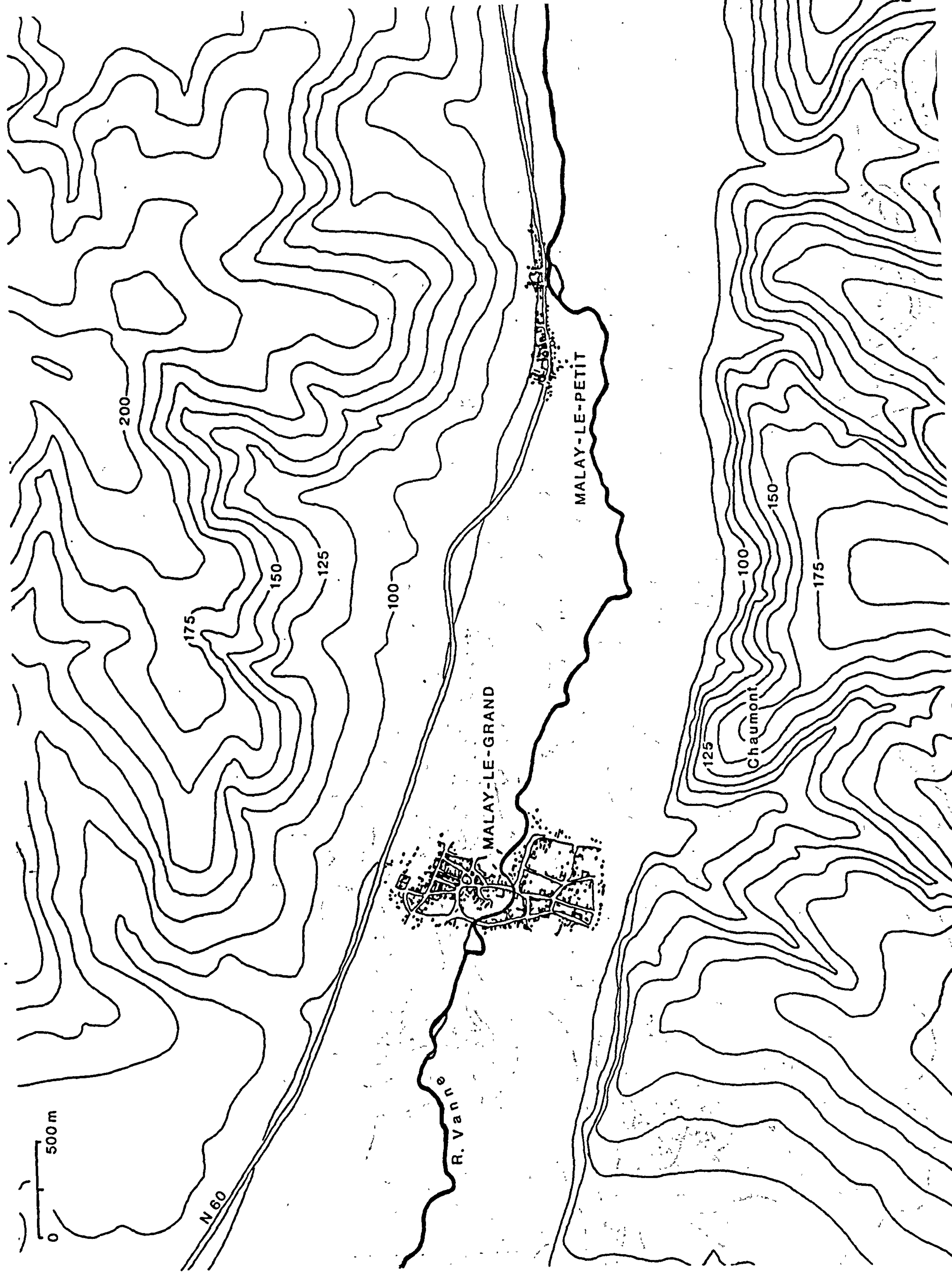


Fig. 4.16 Map of the area around Malay (from IGN 1:50,000).

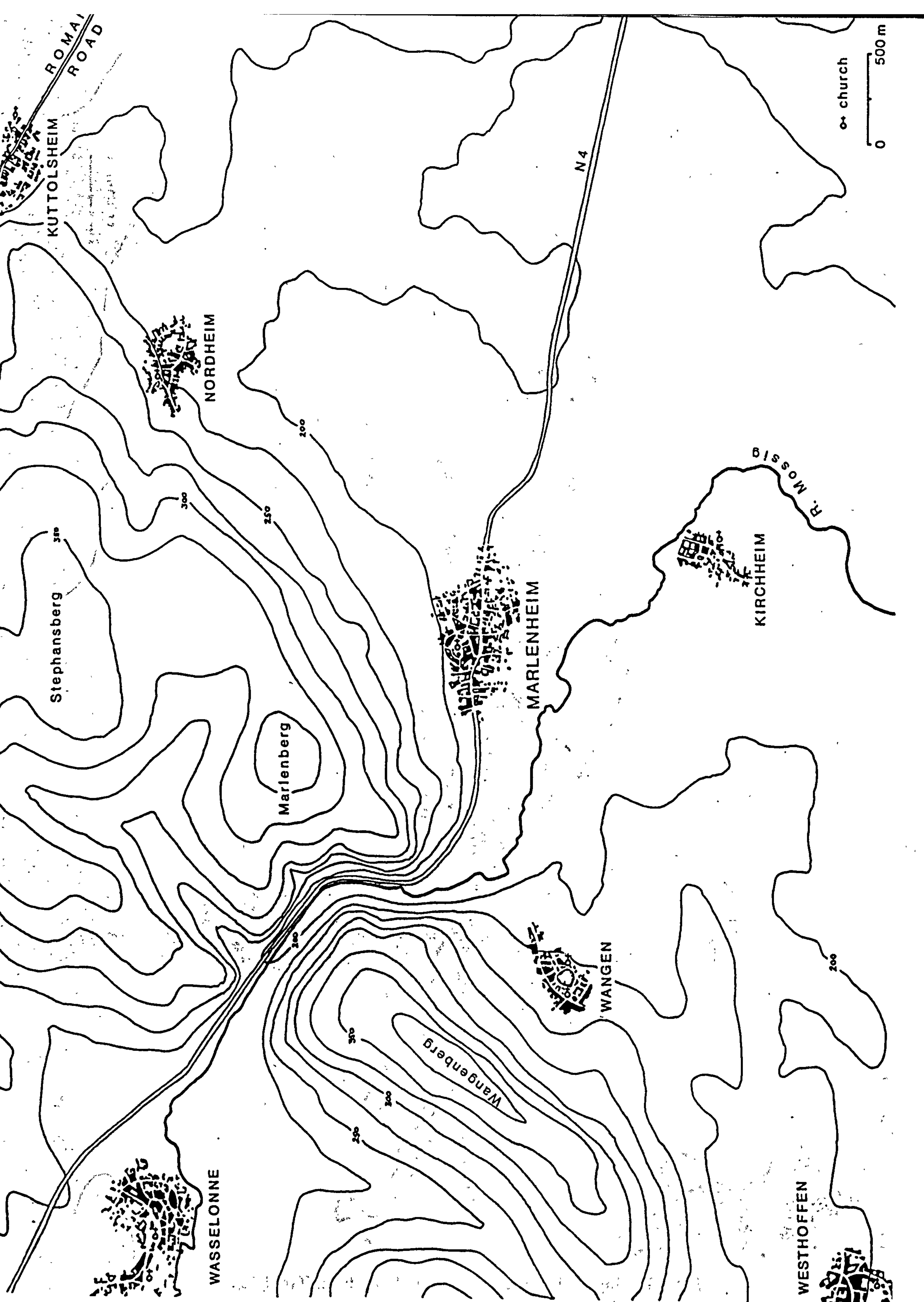


Fig. 4.17 Map of the area around Marlenheim (from IGN 1:50,000).

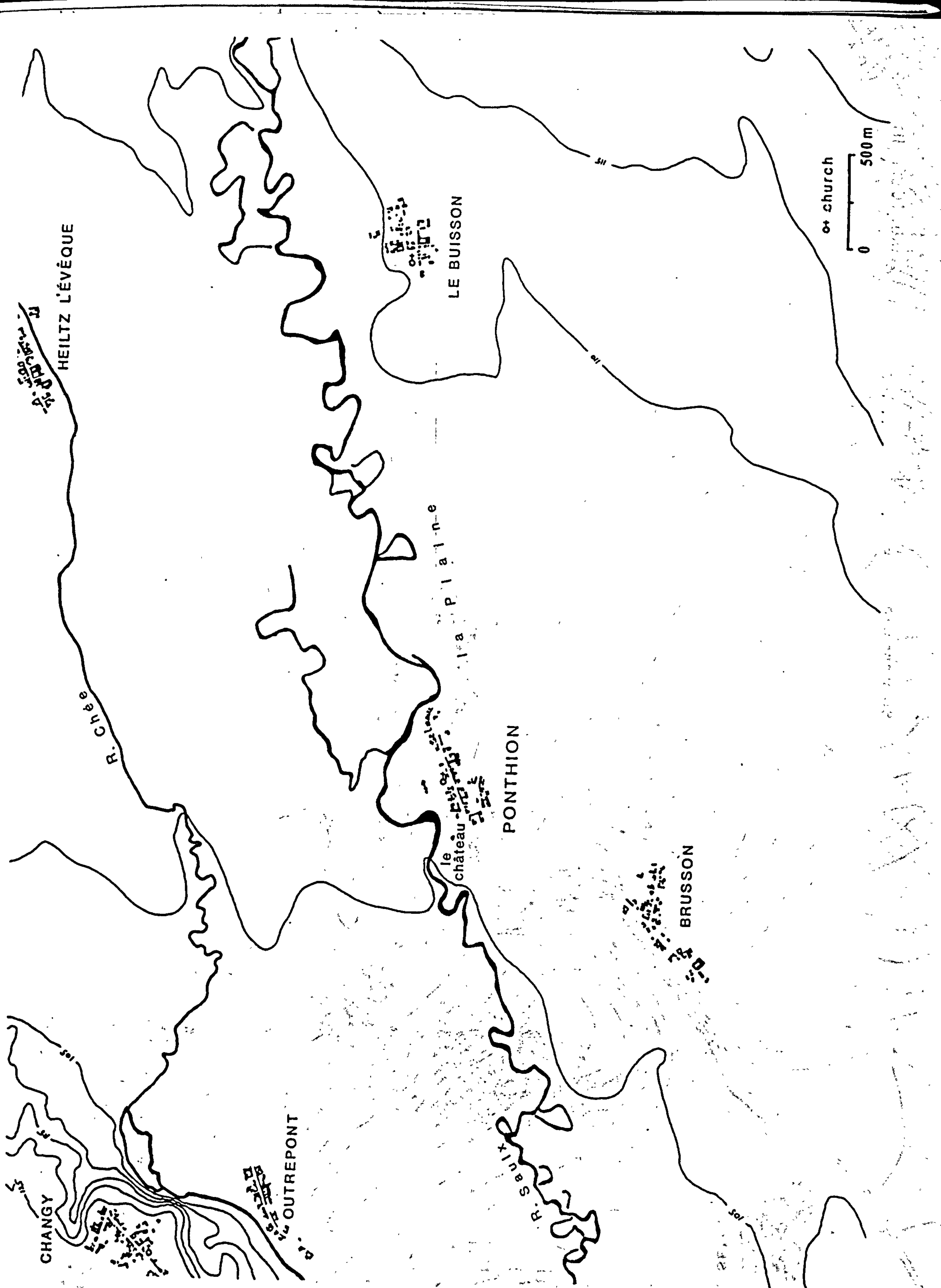


Fig. 4.18 Map of the area around Ponthion (from IGN 1:50,000).

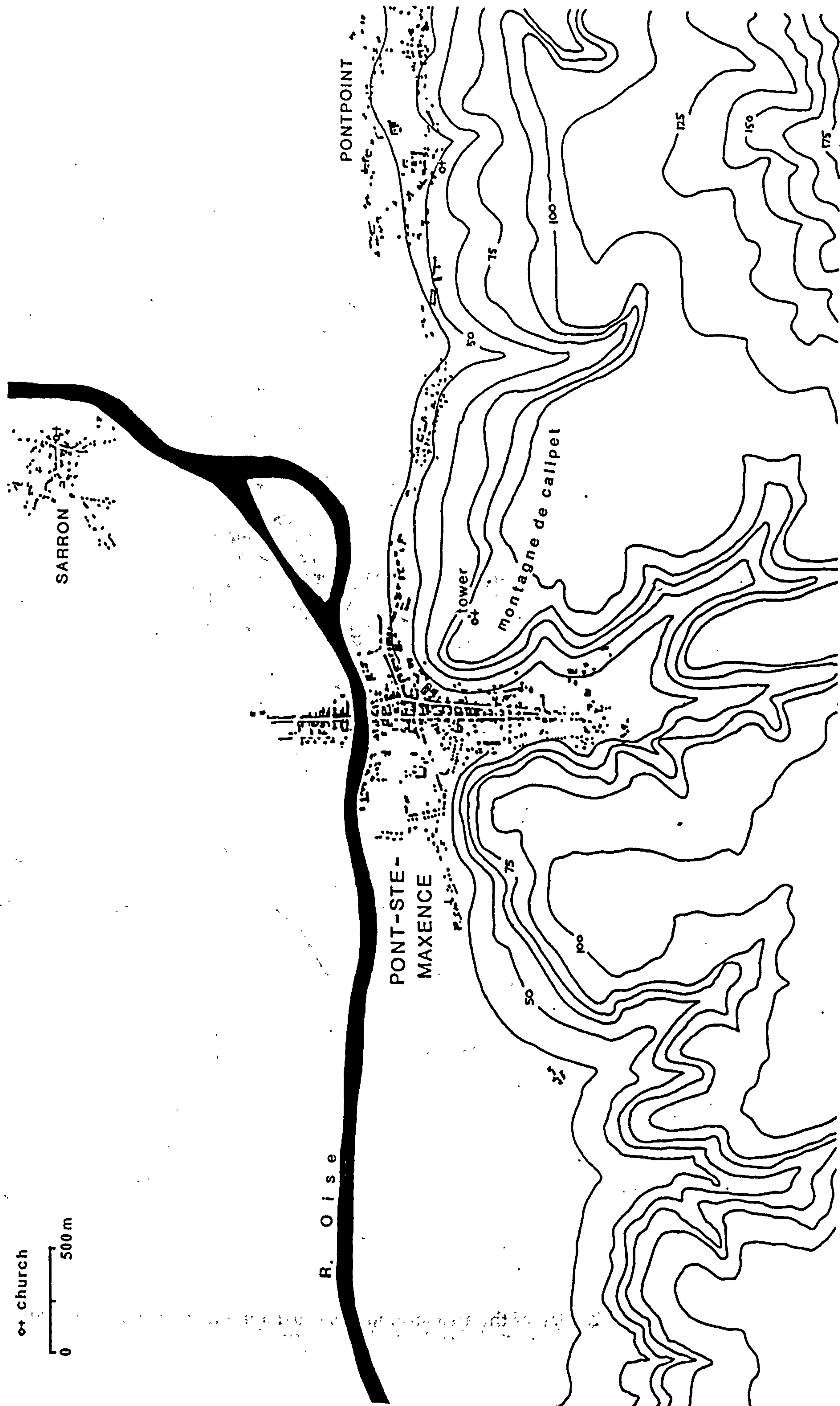


Fig. 4.19 Map of the area around Pont-Ste-Maxence (from IGN 1:50,000).



Fig. 4.20 Map of the area around Ver-sur-Launette (from IGN 1:50,000).

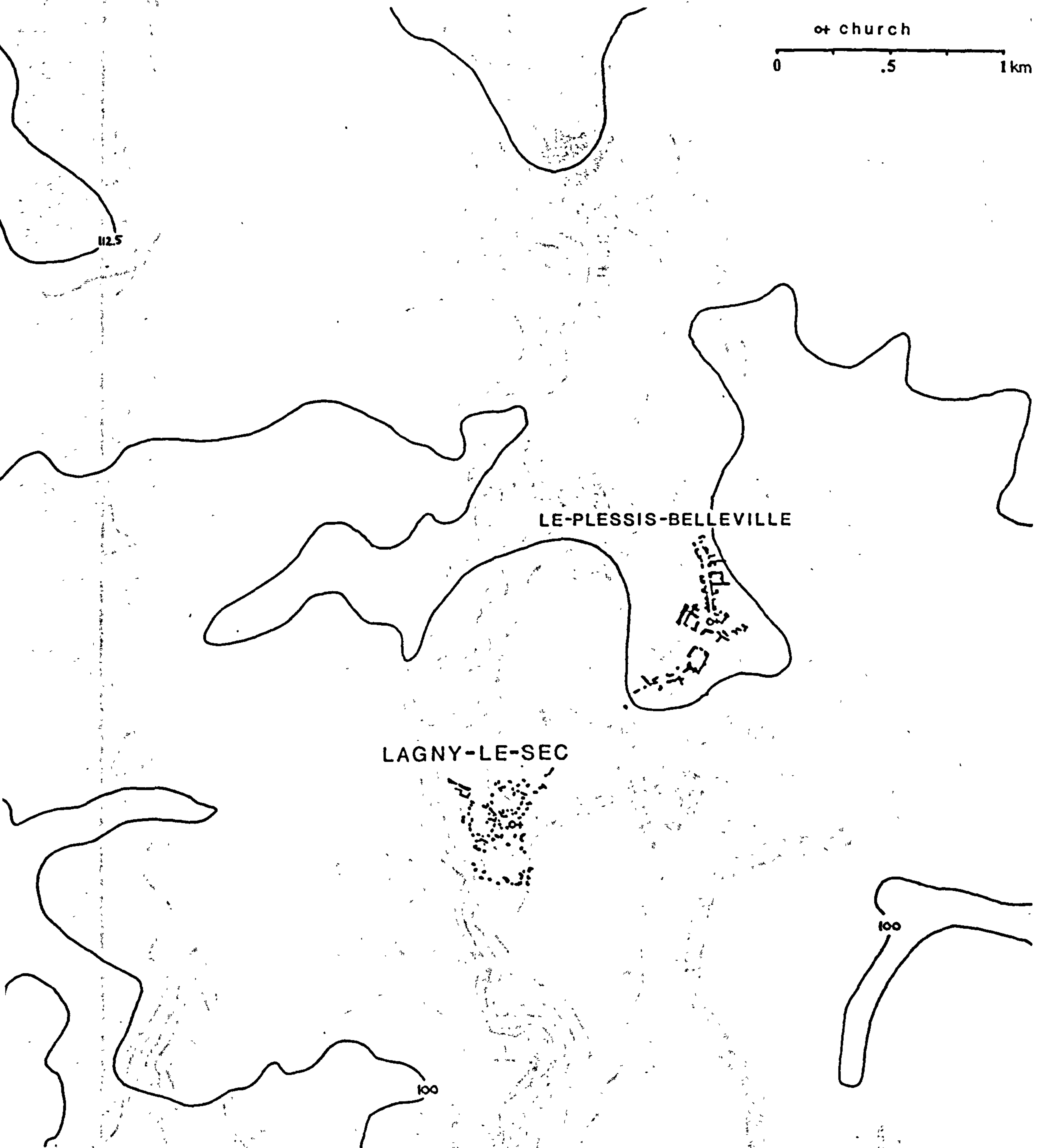


Fig. 4.21 Map of the area around Langy-le-Sec (from IGN 1:50,000).

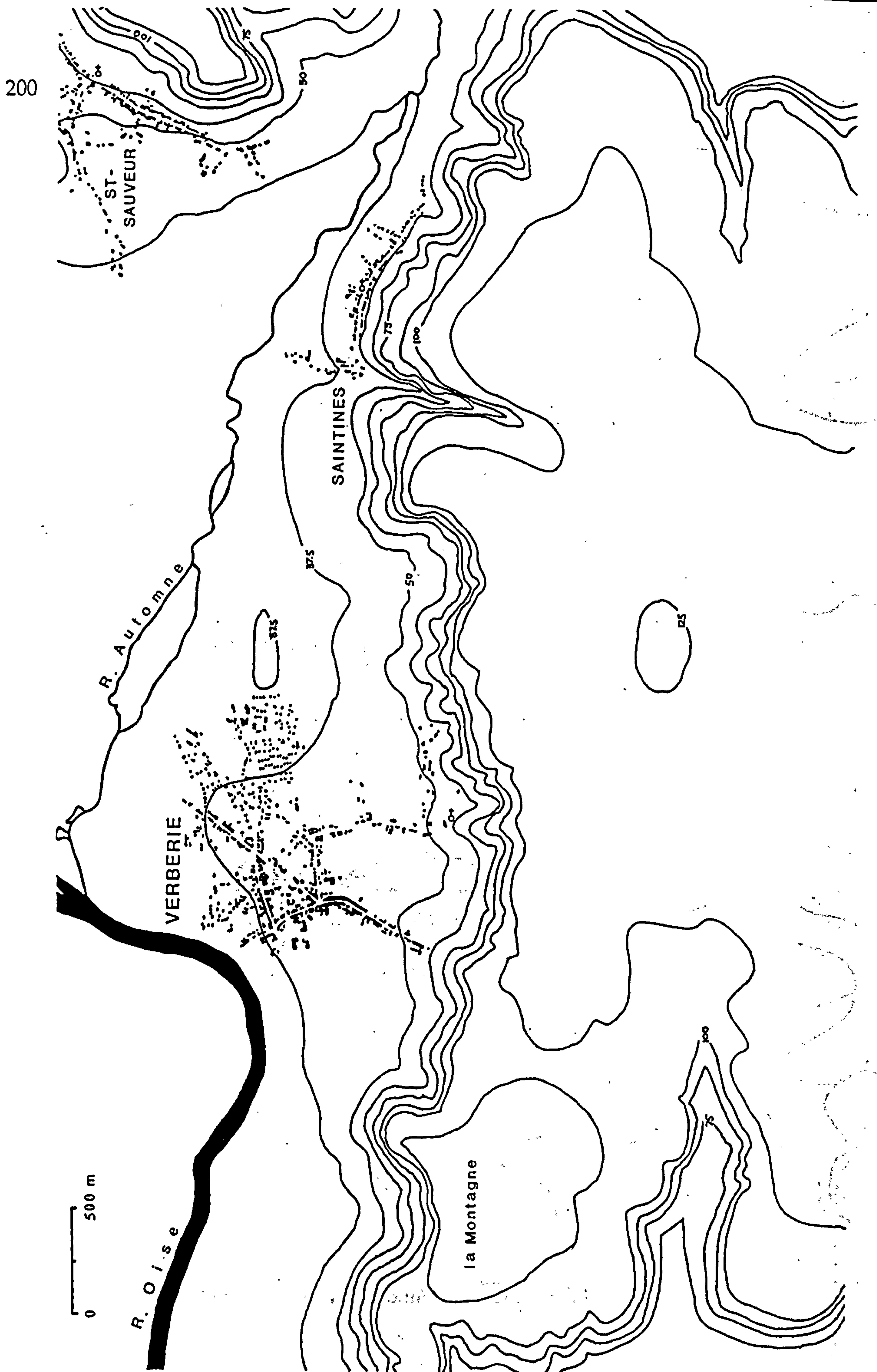


Fig. 4.22 Map of the area around Verberie (from IGN 1:50,000).

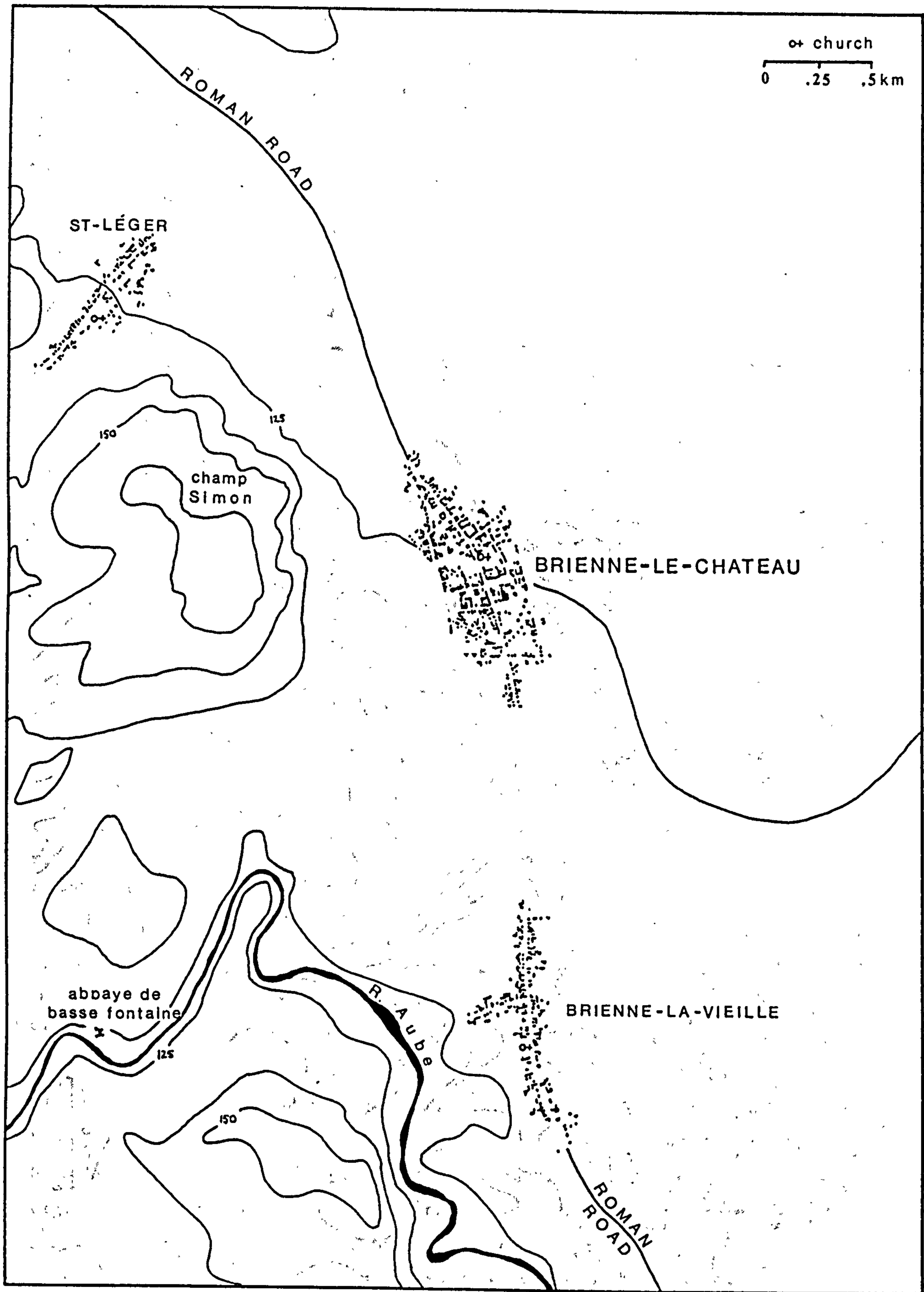


Fig. 4.23 Map of the area around Brienne-le-Château (from IGN 1:50,000).

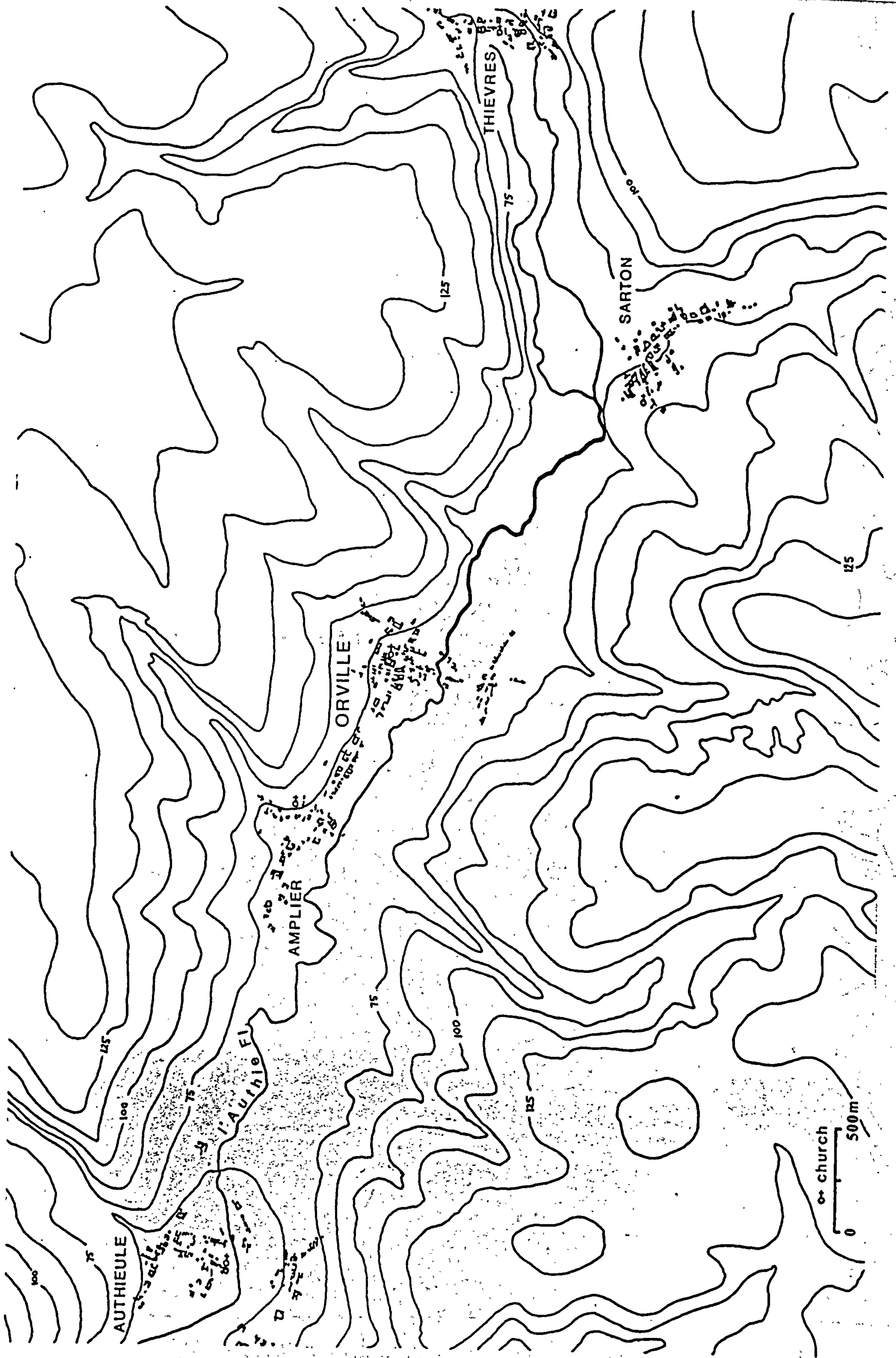


Fig. 4.24 Map of the area around Orville (from IGN 1:50,000).

classical Latin *cohors* meaning 'the space around farm buildings' or 'a yard' which in classical Latin sources can be found used for cattle and even chicken. We will see, however, that the term was used in Carolingian sources almost exclusively as a generic term meaning roughly the equivalent of villa. Scholars would like to see this as a case of *pars pro toto*. The term, however, was not a common one in the Merovingian period. It does appear in the barbarian laws and is argued by Dolling (1958, *passim*) to be interchangeable with villa. Admittedly the term appears in *Lex Gundobada* (54.2; 92.1; 92.2; 92.3), thus proving its early use. It does not, however, appear in the writings of Gregory of Tours, the will of Bishop Bertram, nor in the history of Fredegar.

The general acceptance, and a hazy one at that, of such a courtyard led Wallace-Hadrill (1960, 25) to translate '*Cumque illa eum in aula venire cerniret . . .*' as 'She saw him coming through the courtyard . . .'. Of course, 'coming into the hall' would have been much better, especially as on leaving the *aula*, Columbanus crossed the threshold (*limitem*) and the building shook (*Fred.* 36). One major disadvantage to the translation 'courtyard', even if the Merovingian sources were more explicit, is that it conjures the image of an open air, preferably rectangular area enclosed by buildings along most or all of its sides, thus forming a yard as one might imagine on a farm or a university quadrangle, in short like the central courtyard at the Roman villa Pfalz (fig. 4.11). However, the court as Dolling imagines it and as it is described in Carolingian sources, is an area lying beyond the buildings. It is the space within a perimeter enclosure, and scattered across it might be a great number of buildings or no more than an ornamental garden.

Dolling (1958, 8) believed that the barbarian laws show that a fence was an essential characteristic of the Frankish villa: '*wesentliches Merkmal des salfränkischen Hofes ist der Zaun.*' The fence figures conspicuously in many codes (*Lex Salica* 34.1; 16.5; *Lex Ribuaria* 47.1; 73.3; 73.4; *Lex Gundobada* 27.1; 27.3; 27.4). There are two anecdotes related by Gregory which would appear to locate such fences as described by the laws directly outside a villa *domus*. When Chramnesind killed Sichar he hung the naked corpse from what Thorpe translates as 'a post in his garden fence' (*sepis stipite*) (*HF* 9.19). At Chelles Fredegund had the head shaved of a girl for whom her son, Clovis had taken a fancy, and had her 'tied to a stake which stuck up outside Clovis' lodging' (*sude inpositam defigi ante metatum praecipit*) (*HF* 5.39). In the Frankish laws the fence appears as wooden posts joined one to another by horizontal poles, perhaps held with wickers. The Burgundian lawcodes make it clear that such fences were designed to protect crops, hay, and vines from animals. There is no hint that these fences in the barbarian lawcodes specifically enclosed villas.

A courtyard or inner court is explicitly mentioned on three occasions when Gregory used the term *atrium*. Gregory sought Bishop Salvius in the *atrium* of the house at Berny-Rivière where Salvius had been staying (*in atrio Brinnacensis domus*) (HF 5.50). Representatives of the count of Bourges went to a property of St. Martin to collect men for the army. The steward tried to prevent them, but one marched into the *atrium* of the house (*ingressus est atrium domus*). He collapsed and explained to the steward that when he had entered the *atrium domus* he saw there a man who held a tree, the branches of which grew to cover the whole *atrium* before he, the malfactor, was stricken by divine vengeance (HF 7.42). Finally (HF 3.15), when Leo and Attalus decided to escape from their Frankish lord one night, Leo

found the gates of the *atrium* unfastened, by a miracle, for he had secured them at nightfall with wedges which he had driven home with a mallet to keep the horses in . . . (*invenitque ianuas atrii divinitus reseratas, quas in initio noctis cum cuneis malleo percussis obseraverat pro custodia caballorum*).

Given how little Gregory says about architectural details of *domus*, it is not insignificant that he should mention an *atrium* of three separate *domus*. What was the architectural inspiration for such a term? These anecdotes suggest the *atrium* was an external rather than an inner court, unlike the *atrium* of a Roman house. Using the term *atrium* Gregory was probably invoking the image of contemporary church *atria*; square or rectangular enclosed spaces at the eastern or western end of a church which often formed an open portico around three or four sides. Church *atria* figure regularly in Gregory's writings (James 1981). Most commonly an *atrium* stood at the western end of the church and thus lay in front of the main entrance; an enclosed courtyard in front of a Merovingian *domus* is not difficult to imagine. Just such a courtyard will be seen appearing in some of the royal Carolingian villas, later.

It is interesting that a recurring theme, and almost the only architectural description Fortunatus offers in his poems, is that of the triple arcade or porticus: rows of three columns at Mediolanum, a triple arch at the *casa* of Vereginis, and even the wooden *domus* was surrounded by 'a high, severe, and square porticus'. In his section on Mediolanum, Böhner (1958) discusses Egger's suggestion of a three-aisled hall and Sauerland's three-storeyed building. He rejects both and opts for something like a three-winged Roman villa, such as Nennig, with an arcaded corridor fronting each of the three faces. While a three-aisled hall would fit the Mediolanum description, Böhner's interpretation has the advantage that it fits all the other descriptions as well, although none figure in his discussion. Indeed, I would go further and suggest that such a winged villa could be

described as having an *atrium* by Gregory if a wall ran from one forward projecting wing to the other, forming a little open courtyard before the main entrance. We might also consider that the villas described did not necessarily form a continuous building, but may have been composed of three separate buildings forming three sides of a rectangular enclosure.

Archaeology shows us an enclosure wall at many sites. At the camp de Larina the rampart ran for a kilometre, or two-thirds of the total circuit. At Reccopolis the same area was enclosed but the rampart was, presumably, continuous for the 1.6 kilometres. At Carouge the ditch may have enclosed a similarly large area, which may have been characteristic of Merovingian villas. Following Blondel, Gabriel Fournier (1962, 358) claims that early medieval 'fortresses' might attain vast dimensions, of 5 to 20 hectares, thus allowing them to contain fields within the enclosures. The *villa* Solignac near Limoges is described in the *Vita Eligii* (1.16) as 'enclosed by a circular wall, not of stone but by a rampart [of earth?]' (*ambitur vero in spherio muro, non quidem lapideo, sed fossatum sepe munitum*) supposedly ten miles in circuit (*decem fere stadiorum habens spatium in circuito*). Theopolis was said to be enclosed by walls according to its inscription, which could conceivably have reached these proportions; Langmauer did. Whatever its date, the Merovingian nunnery of Ste-Odile (Bas-Rhin) was founded on a villa within the so-called 'heathen wall' of over 10 kilometres in length (fig. 4.25). In chapter two I suggested that the *castrum* Chastel-Marlhac, on a natural plateau of 40 ha., was perhaps a villa. The suggestion is even more likely of Ronzières where again was found a large enclosure (fig. 2.14).

There is no need to reiterate all the interpretations I put forward in another paper on these enclosures (Samson 1987), except to say that I argued against their military function,³ and for their ideological expression of a cosmological order. The most tenuous argument put forward was that the enclosures had imperial connotations. The relationship between villa enclosures and town or *castra* walls seems more reasonable. But the expression of property ownership is perhaps still the most immediately acceptable functional explanation for the enclosures (*ibid.* for citations of the lawcodes).

Another reason for the enclosures, large or small, around Merovingian villas derives from the social implications of the spatial relationship between the residences of lords and peasants (Samson 1987; 1989; forth.). Here I shall

³ The paper attempted furthermore to demonstrate that violence was endemic in Merovingian society, but that survival, as always in feuding societies, was a matter of politics and not physical defences. The argument that massively long enclosure walls like those at the Camp de Larina were not defensive has failed to convince John Percival (forthcoming, see chapter 4, footnote 1).

widespread agricultural wage labouring, one finds that landlords might even relocate peasants to sites well out of sight. When the tie of dependency was reduced to the medium of wage and no longer personal, the spatial separation of lordly residence and peasant dwelling was complete. Between the two came medieval serfdom and a sort of half-way stage. Peasant villages might huddle around the manor or castle, but the immediacy of the Roman situation was gone (see Samson forth. for this argument in full).


In addition to this long-range view of social and architectural change we can take the very specific, particularist case of Roman villa enclosures. Much neglected by Romanists, I have suggested that they were less designed to keep wild animals or thieves out, but the dependent labourers in. The villa enclosure wall acted not precisely like a prison wall, because the inmates spent most of the day beyond it and physically it could not have acted as a great obstacle to escape. It did however act to define where one should and should not be at times of curfew. It defined when a slave or *colonus* was trying to escape for it was impossible to claim that he or she had accidentally climbed the wall or unlocked the gate. Thus the enclosure wall was a simple device that unequivocally imparted the knowledge that laws had been broken by those crossing them.

In light of this interpretation the assumed commonplace enclosure around Merovingian villas takes on added significance. It implies that the control of some peasant labour was great, that slavery, even if somewhat milder in form than the worst of Roman chattel slavery, still played an important role in Merovingian villa economies. This is in keeping too with the frequent admission of French medievalists that slavery was important until well into the Carolingian period. In Visigothic Spain, for example, some half of all the enactments in the lawcodes – and they are large by barbarian standards – contain reference to slaves.

Such slaves might have been domestic like Leo, or agricultural like Attalus, both of whom we met above in Gregory of Tours's tale of his captured noble relative rescued by one of the family slaves. Such slaves would presumably have lived, like Roman slaves, either in the lord's *domus* or in some outbuilding within the courtyard. Such an arrangement is presumably to be found at the Camp de Larina, where the peasants' '*cabanes*' are located within the great enclosure wall.

The walls at the Camp de Larina could be seen as having defined and demarcated the miniature world of the villa owner. His dwelling, his serfs or slaves, and his chapel, were all to be found within this enormous enclosure. To molest these villagers was not simply to wrong their lord and protector, but also to penetrate his world, delineated by the enclosure wall. The converse to protection is also implied: the dependency of the villagers is also expressed by



 Fig. 4.26 Brebières (A), 'Les Octrois Ensisheim (B), Riedisheim (C), and Proville (D), excavated late Merovingian-early Carolingian settlements (various orientations; 1:1,000) (after Demolon 1972, Schweitzer 1984, and Florin 1983). Note that all sites are composed entirely of Grubenhäuser unlike excavated settlements in Germania (compare fig. 5.2).

their inclusion within the lord's world. It may have been that rather than as defence, enclosures were considered necessary by landowners to emphasise their lordship and their jurisdiction over anything that happened within.

Thus Percival's process of nucleation has implications for the social organisation of the peasants grouped around a Frankish villa. The *vicini* discussed above would appear to have been the dependent peasants of a noble Frank. Where and how Merovingian peasants lived is not easily answered by the textual sources, for they make only fleeting appearances, housed in a *casa* or *tugurium*, 'hut'. When they do appear it is primarily as a setting for a miracle in a saint's life or as the appurtenance of a villa in a charter. Precious little information is generally to be gleaned except that the authors saw them as mean or squalid places.

Conversely, early medieval archaeology probably informs us better about peasants' homes than about those of nobles, unlike Roman archaeology. The *tugurium* of documents is quite likely the *Grubenhaus* of excavations, and excavations in France have yielded little other than *Grubenhäuser* in this period. The excavated settlements of Mondeville, Brebières, Riedisheim, 'Les Octrois', all share in common the fact that they were composed almost entirely of *Grubenhäuser* (fig. 4.26).

It is possible that these villages represent the settlements of relatively autonomous peasants, but this seems unlikely. In the first instance the common farmsteads of Germania appear to have large timber-framed halls as the main dwellings (see next chapter), as do most Anglo-Saxon settlements. The farmsteads of independent peasants in the Frankish realms were unlikely to have been so much less substantial.

That they were settlements of *coloni* or *servi casati* seems more likely, and as noted earlier, Brebières actually lies only four kilometres from the royal villa of Vitry-en-Artois; the excavator postulates that this was one of the villa's settlements of dependent peasants.

Concluding Remarks

Merovingian villas have undergone very little critical study. Archaeologically they remain sufficiently incognito that the best excavated site, the Camp de Larina, surfaces in articles and books as a fortress or *castrum* but never as a villa. The temptation to see the inhabitants of sixth- to eighth-century Gaul dwelling in timber halls is irresistible to most. It neatly explains the lack of good evidence for fifth-century and subsequent occupation of Roman villas, it coincides with the domestic architecture that has been dug up by the spade and trowel (although

that evidence comes largely from England and Germany and is happily borrowed on an assumed Germanic similarity of cultures), and it suits the picture of Merovingian inferiority in comparison with Carolingian economics, which is widespread.

In this chapter I think I have shown that royalty, at least, resided in stone buildings, and that the nobility probably did too. Urban stone buildings of more than one storey not only survived from previous centuries, but were kept up and built anew (chapter two). Many aspects of Roman villa architecture and topographic setting appear to have remained common. Of course the building tradition was a pale reflection of former Roman sophistication. Column capitals were probably more often than not *spolia*, and continuous long ranges were replaced by free-standing buildings. Nevertheless, the sophistication was sufficient to make Yeavinger appear humble in comparison. But it is to the Merovingian's Germanic neighbours in the east we turn to next.

GERMANIA

Chapter Five

Fourth- to Seventh-Century Germania

Settlement Geography

The settlement geography of Germania, unlike Gaul, has little documentary evidence to illuminate this period, but has a slightly better archaeological record. To gain an insight into the settlement geography of Germania, we might begin with the cemeteries. What do the one thousand cemeteries of Baden-Württemberg show us about the settlements (fig. 5.9)? There is the perennial problem of knowing the exact relationship of cemeteries to their settlements when the latter are only poorly known. Nevertheless, there is much to suggest that a settlement lay almost immediately adjacent to its cemetery. This evidence is particularly clear when we have a shift of burial place in a very small geographic area with a sharp chronological break, when a new cemetery frequently appears near-by. In Alamannia these moves appear to coincide most often with changes in burial rites. Thus many small cemeteries began in the fourth century, some moves occurred around 600, but mostly burial places moved when *Reihengräber* burial practices ended in the early eighth century. Thereafter burial grounds remained generally fixed, for this last move was frequently to a recently established church. It would appear that often a single, settled community used first one burial place then another. This is most clear in the last phase of the *Reihengräber* when found beside churches, next to which we sometimes find the settlement itself. At Burgheim burials extend to the corner of the settlement uncovered by excavations. At Breslingen we can see settlement, burial, and church at a glance (fig. 5.1).

Writing wider settlement history from cemeteries does pose a major problem: the relationship of burial practice to archaeological visibility. Some scholars have pressed the burial evidence to reveal changing settlement through time. Schwarz believed that Carolingian burial evidence in north-eastern Bavaria reflected an extension of settlement into hillier and less fertile areas. His work and his maps are regularly displayed as examples of how burials can be used to write settlement history (e.g. Jankuhn 1977). However, the burial customs Schwarz

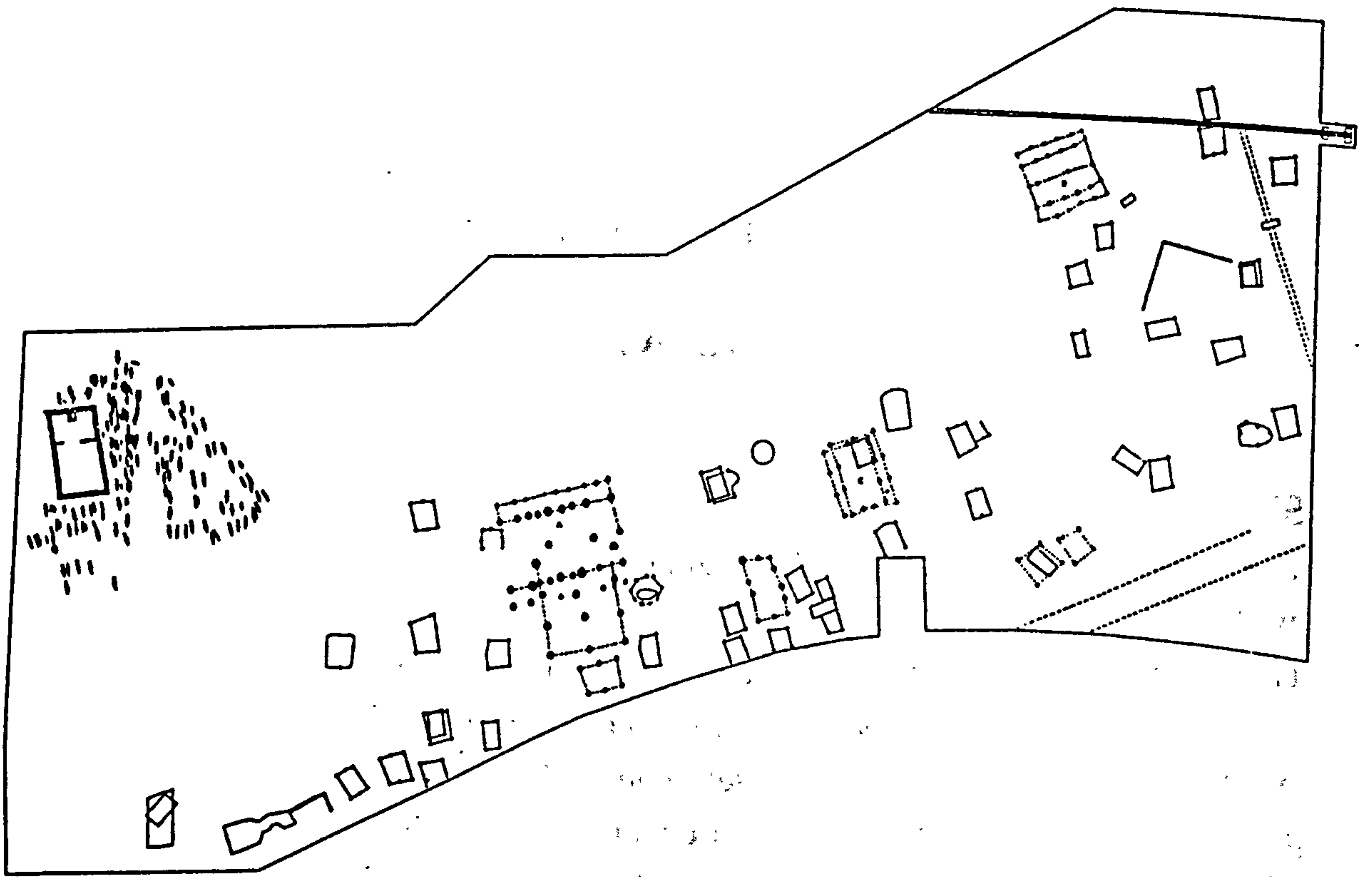


Fig. 5.1 Breslingen Carolingian village (1:1,000). Represented in the excavated portion of the settlement are a possible village fence, large timber halls, small six- and four-post huts, church, and cemetery (after Guyan).

discussed changed through time. His maps probably reveal the spread of the burial custom rather than the fresh pioneering efforts of Carolingian farmers. This seems especially probable given that his maps would otherwise reveal an abandonment of the more fertile areas by Carolingian farmers, whereas these later lacunae in the distribution of cemeteries surely reflect the abandonment of burial with grave-goods.

As burial practices change and become easier or more difficult to recognise by archaeologists, the impression is often mistakenly given of a countryside increasingly or decreasingly densely populated. The problems of visibility notwithstanding, the *Reihengräber* evidence can allow us to say that the majority of people lived precisely where one would expect, near fertile well-drained land. In addition, we can say that most people lived far from the nearest suitable site for a hillfort. The burials may tell us that the countryside was as densely populated as during the brief occupation by the Romans (see later in this chapter and figs. 5.8 and 5.9).

The cemetery evidence can also suggest the size of the settlements, if we

allow that each cemetery was indeed used by only one settlement. Attempts to work out membership of different farmsteads have been made (Ahrens 1978) and attempts to recreate the composition of the group according to age, sex, and social standing as it would have been in life at a given moment, a snapshot of the living group, so to speak (Martin 1978). The latter is particularly untrustworthy for it is based on the assumption that individual graves can be dated to within five years of actual burial. Nevertheless, these attempts have strengthened the calculations of Donat and Ullrich (1971) who argue for 20-25 members of each *Hof* and average settlements composed of two or three of these large farmsteads. The evidence from excavated settlements does nothing to change the impression. Thus despite frequent references by archaeologists to *stadtähnliche* settlements, there were no towns, even in the most general sense of a settlement with a large number of inhabitants. 'Town-like' is often applied to Runder Berg or Feddersen Wierde, but even if we even see two hundred inhabitants there we are in danger of erring on the large side.

What evidence we do have of these farming settlements on well-drained soils near *Reihengräber* cemeteries is being steadily increased. The quickest introduction is that of Chapelot and Fossier (1985), but the essential starting point is the work of Peter Donat (1980). His seminal work, *Haus, Hof und Dorf*, includes a sixty-page catalogue of sites in German-speaking countries. From it we find that in the fourth- to eighth-century *Reihengräber* areas of Alsace, Baden-Württemberg, Bavaria, and Switzerland farmsteads have been excavated at Berslingen, Burgheim, Deffingen, Epölling-Mühlthal, Geislingen an der Steige, Haunersdorf, Inningen, Kirchheim, Leibersheim bei Riedisheim, Merdingen, Sasbach, Schwabmünchen, Sontheim im Stubental, Stebbach, Straubing, Wittislingen, and Wulfingen. To this we could add sites from central and northern Germany, including the long-known Gladbach and best-known Warendorf. One might also want to add the farmsteads from the North German Plain. However, this northern area remains largely outside the scope of my thesis and there is evidence to suggest that there were differences in settlement types (e.g. nucleated mound, *terp*, settlements), different house forms (e.g. byre-houses), and different economies (concentration on cattle rearing rather than arable, and thus perhaps the reason for the presence of byre-houses).

The title of Donat's book, *Haus, Hof und Dorf*, nicely summarises the three elements making up the constituent parts of most settlements. *Höfe*, or farmsteads, are seen as basic units, composed of a dwelling and several ancillary buildings. A farmstead might exist alone or a number might be found side by side to form a 'village'. Warendorf is taken to demonstrate best the composition

of one of these large farmsteads (fig. 5.2). A large rectangular timber post-built house is taken to be the main dwelling. Ancillary to this were smaller rectangular post buildings, *Grubenhäuser*, and other varied small timber constructions. The separate parts are taken to compose a whole, enclosed behind a palisade fence. The various ancillary buildings are held to perform various functions, acting as barns, byres, store-rooms, weaving sheds, hay ricks, sties, chicken coops, and quarters for slaves or dependants. Warendorf also demonstrates that for every large farmhouse there was a fair number of dependent buildings. Thus at Burgheim, for one large hall, there were two smaller timber post buildings and at least twelve *Grubenhäuser* excavated. At Gladbach the proportion of *Grubenhäuser* appears to be perhaps too large, but it was excavated before the Second World War.

The term 'village' is a loaded one, for these settlements must be seen in a completely different light from modern villages, which fit into a settlement system that simply did not exist in Germania. The term *Haufendorf* has been coined to express the idea of several neighbouring farmsteads forming a single organisation, but one in which there was no differentiation that we would think of as characterising a 'village', no blacksmith here and miller there. An individual farmstead might be thought broadly equivalent to a Merovingian or Carolingian *mansus*, but there is one possible difference of significance. In Gaul many *mansi* formed part of a wider complex of ownership, exploitation, and organised labour. This may be discernible in the archaeological record.

A comparison of these Germanic farmsteads with those in Gaul (fig. 4.26) at Brebières, Mondreville, Riedisheim, or Proville, reveals the former to be better equipped all round, more like individual, autonomous, and proprietor-occupied farmsteads. They were more likely to be enclosed behind fences which have been argued to mark private property and changes in land ownership from the early Roman Iron Age to the Migration period (Donat 1985; 1987; Samson forth.). They are composed of more varied ancillary buildings, but most marked are the *Grubenhäuser*. In Germania they are generally dependent on larger timber post-built halls, whereas in Gaul many of the excavated villages appear to be composed of nothing else. It was suggested in the last chapter that these Gallic settlements were dependent on seigneurial villas and were occupied by slaves or serfs. Here we should note that the *Grubenhäuser* in Germania are frequently likewise interpreted as slave quarters. If this were so, we might suppose that the villages of Germania reveal more autonomy, less dependence, and less exploitation from a ruling elite.

It is occasionally suggested by the excavators of such Germanic settlements

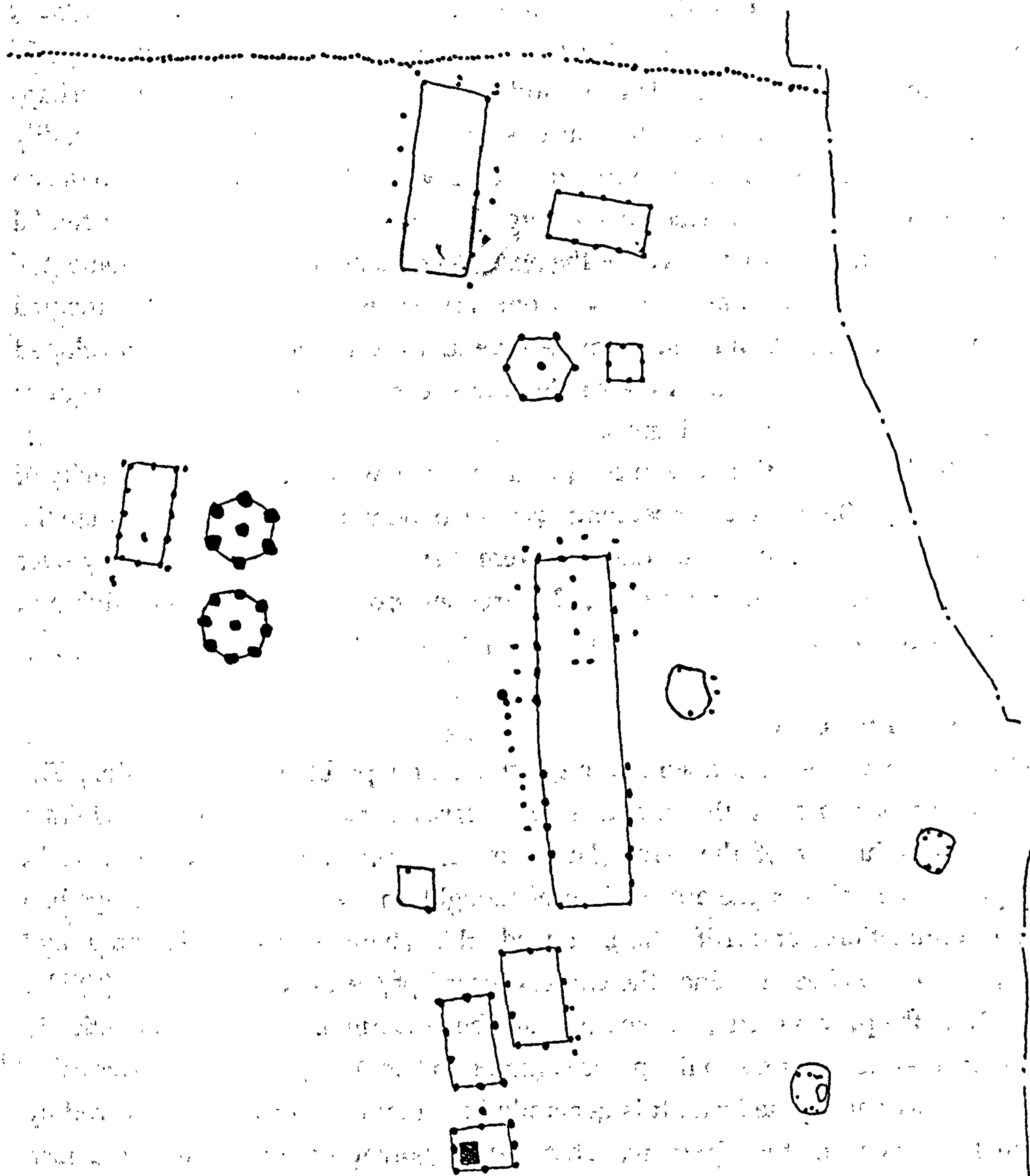
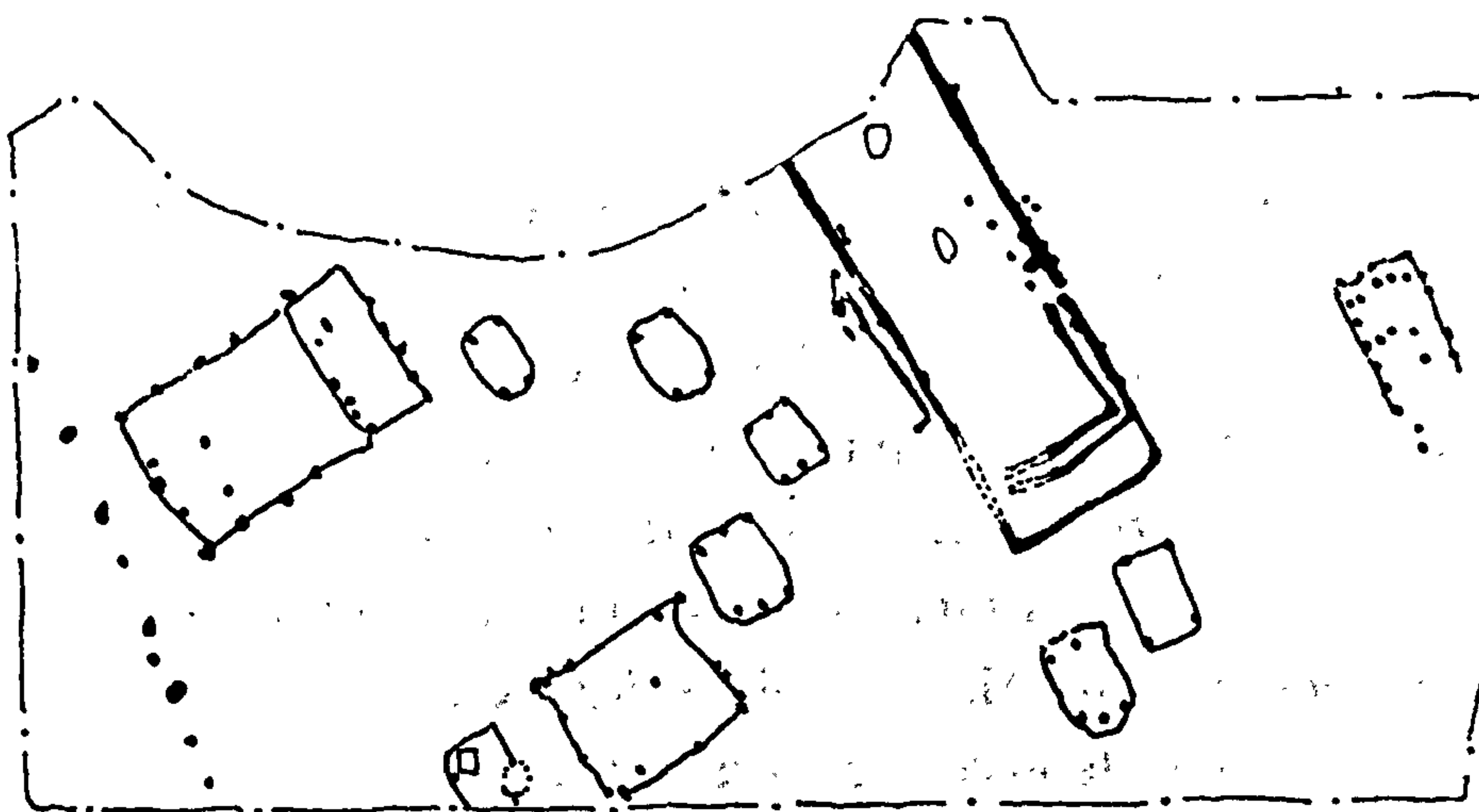


Fig. 5.2 Warendorf and Burgheim, seventh/eighth-century Germanic villages.
 Note the long timber halls absent from the excavated Merovingian settlements in Gaul (fig. 4.26).

that the biggest hall was inhabited by the 'chief' or lord. At Feddersen Wierde Haarnagel's interpretation of one farmstead as the Herrenhof has been widely accepted (e.g. Todd 1975, 106). The restriction of social dominance to within the immediate farming community has similarly been proposed for brochs in Scotland at this period (Foster 1989). It is held by some Marxist archaeologists that it was from such origins in the Migration period that medieval lordship in Germany would grow. Although the evidence is meagre, Donat (1978) has tried to show that the differences between farmsteads of the sixth to ninth centuries and those of the ninth and tenth centuries in Saxony are the result of the growth of feudal exploitative lordship. The later farmsteads are generally somewhat smaller than those of the earlier period and have fewer ancillary buildings. Of course the exceptions in the later period are the large manorial estates. Perhaps most importantly, Donat (1977) also shows that the size of byres steadily increased from the Iron Age to the fifth century AD, but the mean of two dozen head of cattle per farmstead in the Migration period later drops when feudal lords are securely documented for the northern regions. The local development of feudal lords, in the Marxist sense, from farmers is perhaps best documented archaeologically in Denmark. From the seventh to tenth century farms developed that were much larger than their neighbours, earning them the name *magnate farms* from Danish archaeologists.

Charlemagne's difficulty in conquering Saxony was due to the multiplicity of local powers. But were most settlements independent of any greater lord than the most powerful farmer in the hamlet? Were there no nobles with authority over more than a few neighbouring farms? Were there no *potentiores* or *reguli* living in hillforts, in *Burgen*?

Burgen: the Term

Once again we are faced with the questions and problems of definition. The obvious place to start, the *Reallexikon der germanischen Altertumskunde*, offers a condensed history of the term. The entry is confusing and thus, one feels, captures the spirit of the archaeological thought on the term. It has always had the connotations of fortification, indeed, this characteristic is essential and apparently has been so since the earliest record of Germanic languages (Köbler 1972). In the previous chapter we saw how the villa *Burgus* of Pontius Leontius in the fifth century had taken the name common in late Antiquity for what we call a watch-tower along the *limes*. It is generally held that the word was borrowed by the Romans from the Germans. That it was borrowed we know for a fact, although a few have suggested that it came from Greek. Of course if borrowed

from the Germans it does not follow that it had any such technical meaning among them as it was to have among the Romans.

When *burg* does first appear documented among German speakers in medieval times, it is repeatedly used to translate the Latin terms *civitas* and *urbs*. So it appears frequently in glosses of the eighth, ninth, and tenth centuries. It was used to describe Jerusalem; Bethlehem became Bethlemaburg; and most famous of all, Gregory of Tours (HF 9.36) records '*Argentoratensem urbem, quam nunc Strateburgum vocant*'. The Franks settled in the Rhine valley were responsible for giving Strasburg its new name. Walter Schlesinger suggests that a German writer of the eighth century might have understood by *civitas* a settlement with a large number of inhabitants, following Augustine's or Isidore's definition. At any rate it would not have held any overt nuances of fortification, according to Schlesinger. However, any German writer of the eighth century with experience of cities must have known primarily those of the former western empire or North Africa where fortifications were often very conspicuous. Perhaps there was no separating the two features of fortification and numerous inhabitants from the term as used by contemporaries.

In the Romance languages *burgus* did not retain its meaning as a watch-tower, if indeed it ever entered the vulgar Latin vocabulary with such a meaning. It was all but absent from Merovingian texts, other than in place-names, appearing only in the later Carolingian period. In fifty-four pages of well-documented argument Schlesinger (1954) shows how *burgus* and *civitas* represented two different parts of a town in the central Middle Ages, the former something like *suburbium*, very often unfortified but most importantly often connected with merchants. In England and Germany *burgus* could not take on this new meaning because it was already synonymous with *civitas* and thus the new merchant suburbs could occasionally be designated by *wic* or *wik*.

As we saw in chapter two, *vicus* was commonly used to mean small town or village or parish. I ignored there the other common meaning for *vicus*: a town quarter, whether a merchant's or Jewish or saddle-maker's quarter. In Romance languages *burgus* came to equate to *vicus*, so that *bourg* was early used in French for a little town just as *vicus* was used in late vulgar Latin. When used of *urbes* or *civitates*, then either denoted a quarter, much like *suburbium*, although that always had the connotation of being 'outside' or extramural. One weakness with Schlesinger's arguments is that these terms cannot be equated too closely with merchants.

In Germanic-speaking areas Schlesinger's linguistic theory suffers a further weakness. According to his theory the cognates *burgus* and *burg* might be used

with diametrically opposed meanings depending on whether Latin or the vernacular was used. Written, the word should have meant 'merchants' quarter of the town'; spoken the word should have meant 'city'. But how could any rigid distinction be maintained, since the vernacular *burg* was constantly intruding itself into the written text as a place-name element. We cannot see that a technical sense was maintained in sentences containing something like 'the *burgus* of *Brunnenburgus*'. The appearance of *Lundenwic* and *Lundenbyrig* in different sources can hardly be interpreted as meaning that ninth-century London was composed of a town, *burh*, and a merchants' suburb, *wic*. In fact each of the different sources only wished to say 'London'. Similarly, the previous example might have meant no more than 'the city of Bramburg' or alternatively 'the suburb of Bramburg', but the distinction cannot be made out of context.

Even if Schlesinger is right about mercantile connotations of *burgus* in the central Middle Ages, his real historical mistake is to push this linguistic development back from the eleventh, twelfth, and thirteenth centuries into the Carolingian and even Merovingian periods. Richard Hodges (1982) makes the *emporium*, the coastal trading *portus*, *burgus*, or *wik* of the ninth century, a central element of early medieval economy (see chapter two). It is clear that these are sites peculiar to the contemporary settlement geography of western Europe and were not composed of city and traders' quarters at this early period as Schlesinger suggests. According to Hodges, these sites mark the culmination of the politically-controlled and politically-oriented long-distance trade. Using models drawn from economic anthropology, it could be suggested that the period following the decline of these coastal ports was distinguished by trade which could be said to have become capitalistic, even if hedged by a multitude of political controls and monopolies. The increased likelihood of this being correct can be seen in the simple fact that trading activities following the decline of the *emporia* are better understood by modern historians, whereas the nature of Merovingian trade, with its currency essentially consisting only of high value coins without 'small change', is still very poorly understood. Schlesinger's attempts to push the interpretation of city and merchant suburbs back into the Carolingian and Merovingian periods make even less sense east of the Rhine, for until the ninth century there was no currency minted in the German-speaking regions and large quarters for traders and merchants outside German cities is clearly anachronistic. In any case Schlesinger's evidence for the suburb-city distinction which pre-dates the ninth century is meagre and in no case incapable of a very different interpretation.

East of the Rhine *burg* was to remain closely bound to the Latin *urbs* and

civitas until the medieval castle became a common feature on the landscape, so that from the eleventh century it inherited the title alone and a new word *stadt* appeared, which was applied to cities. Köbler (1967; 1972) argues convincingly that the adoption of the term *stadt* developed as a term distinguishing urban centres with peculiar legal rights and was presumably closely connected with a monopoly over trading rights; for the term appears to have developed from *koufstat*, a place where one buys.

The modern term, alas, is no easier to deal with, except that *Burg* cannot be applied to anything which is not clearly 'fortified'. Mildemberger (1978, 22) excludes from his *Germanische Burgen* those enclosed sites which were not 'really' fortified. A conceptual division is made between enclosures for mundane purposes or show and those for 'real' defence, even if it is allowed that in practice the dividing line is not always clear. Therefore an oppidum, hillfort, ring-fort, castle, dun, or even rath could all be called a *Burg*. While this means the term is vague, a series of other terms are also to be found like *Ringwall*, *Rechteckwall*, *Abschnittswall*, or *Motte* which usefully describe the morphology of the site without necessarily being loaded with functional presuppositions. A second series of terms is exactly the reverse. *Fluchtort*, *Refugium*, *Volksburg*, *Gauburg*, *Adelssitz*, etc. all impose a presupposed function on a site by their terminology. These terms fall broadly into two categories, sites that were permanently occupied and those that were not. *Gauburg* is almost never used today, as being anachronistic, for it has connotations of regional administration and thus something like our county capital. Seen as some sort of 'central place' in geographers' terms, the idea still survives in descriptions of sites as '*stadtähnlich*' which would best be rendered by our 'proto-urban'.

Fluchtburg or *Refugium* clearly stands in contrast to *stadtähnlich*. That *Burgen* include the largest and densest settlements as well as purpose-built deserted centres reveals how difficult the term is. That it is accepted that these empty refuges were built in identical fashion, both in construction technique and in form, to densely-populated forts (Fehring 1987, 142) raises in my mind considerable doubt about the validity of the refuge explanation.

There are various reasons for the proposed refuge explanation of these hillforts. One logical source of the idea comes from breaking down sites into functional components. As will be seen later, German scholars are convinced that a fort formed an optional component of royal palaces. Seen as an element that was expendable, it is easily seen further as a component that might be placed near the palace. It has become a spatially-floating, optional extra. I do not believe there is good evidence of palaces or villas contemporary with unoccupied nearby

forts, but that remains for a later chapter.

Another source for the refuge explanation comes from archaeology. The general paucity of settlement evidence from within rampart enclosures is often taken as positive evidence for the lack of settlement. It is saying nothing original to note that until recently, the majority of excavations have concentrated on the visible ramparts, particularly the gateways, and that when internal areas have been investigated, seldom is even ten per cent of the area uncovered. In chapter two I drew attention to the fact that in Belgium, even when indisputable evidence of occupation has been found, it is deemed 'impermanent', although archaeologically the capacity to distinguish in these cases between inhabitation for ten months and ten years is lacking. A salutary exercise might be to gather together our *positive* evidence for settlement on motte and bailey sites and see if a similar picture of uninhabited refuge does not fit these too. It can scarcely be coincidental that almost no site which has been extensively excavated has been interpreted as a *Fluchtburg*.

Weidemann (1972, 74) offers one of the few reasons why *Fluchtburgen* might exist. Villages and farmsteads of the early medieval period were not fortified, according to Weidemann, because they did not have enough inhabitants to offer adequate defence. Therefore, a number of communities built and used *Fluchtburgen* which alone could offer sufficient protection. Such an interpretation is unsatisfactory for a number of reasons. Firstly, Weidemann himself notes (*Führer* 6, 52) that late Carolingian-Ottonian fortifications were small enough that only thirty to forty people were needed to defend them. So why were similar sites not built instead of *Fluchtburgen*? And what does Weidemann mean by 'adequate defence'? Fortifications of any type were quite useless to local farmers when faced by a Saxon, Viking, or Magyar army. Even Carolingian armies could be defeated by such opponents. If the early medieval Germans were attempting to protect themselves from their own society's endemic violence, then one should see it in terms of feuding between farmsteads in which case the great fortifications were unnecessary. If, on the other hand, there were chieftains organising warfare on a larger scale, then the *Burgen* were created by them, not by local farmers clubbing together to raise their communal hideaway. It is important to remember that the nature of violence is dependent on the nature of social organisation. Weidemann has effectively given us a picture of a 'simple', relatively egalitarian society which faces violent attacks commensurate only with a relatively hierarchically organised society. Such a situation of peoples faced with violent aggression from a society considerably more complex does occur in early medieval Europe in the instance of Carolingian expansion across the Rhine,

which was a paler version of the more marked difference in social organisation between the Roman empire and its barbarian neighbours. Even accepting that there were possibly times and areas for which Weidemann's scenario is not completely inappropriate, it still does not convince me that the constructions were regularly left empty.

The *Fliehbürg* is clearly the product of a German academic tradition. We saw in chapter two that the idea has been present in German academic circles for over a hundred years. Belgian archaeologists have adopted the idea, but the concept remains rare in French and British work. Among British archaeologists it is uncommon to assume that fortifications were built in post-Roman times as secure defences, but were then left empty by a cautious rural valley-dwelling people, and occupied only when marauders approached, although this interpretation is accepted by some (D. Hill, pers. comm.). How often do British archaeologists imagine a Celtic warlord, instead of an Alamannic *regulus*, commanding the construction of a twenty hectare fort with gate towers and earth and timber walls more than ten metres thick for the future safety of his people, and then leaving it empty? The result sounds impossibly like a modern bomb or fall-out shelter.

In chapter two I suggested that flight by a frightened rural population to a place of comparative safety that was remote, inaccessible, or more readily defensible, was inherently sensible, but that wherever this might be thought to best fit the archaeological evidence, one would find that the site revealed no traces of having been intentionally constructed as a refuge. A remote hill to which the locals might flee, therefore, was not a *Fluchtbürg*, it was simply a remote hill.

Finally, among the terms are those with implications for the section of society which used the site. *Volksbürg* is used by Marxist archaeologists to mean that the fortification was used by the people, but it is used rather rarely and partially as a historical accident. The tendency today is to equate *Volk* with a state, thus class society, so that it is inappropriate, historically, according to Marxist archaeologists, in the period before the eighth century. *Stammesbürg*, the fort of the tribe, is thus preferred. It is readily admitted that construction might be directed by the tribal aristocracy, but this is taken as further evidence of their inability to exploit others, for the ramparts are used by all. Schuchhardt, who first used the term of sites in lower Saxony, is thus treated with some respect by Brachmann (1983), although he saw these sites as rather more like a medieval castle but capable of holding a much larger number of refugees. Thus rather than a lord building a private fortified residence, in which only a small portion of the surrounding dependent peasants could shelter in times of need, he built a *Volksbürg* where he resided, but a large portion of the country folk could also be

taken in, although normally they lived elsewhere.

The emphasis may be different, but in practice what Schuchhardt was describing was effectively an *Adelssitz*. This is another term used of early medieval *Burgen*. It is very clearly connected with medieval castles with the sense of a family caput. Because *Burg* is also used of what we call 'castles', the word has always been evocative of feudal lordship.

Later in this chapter we will look at the debate that equates fortifications with lordship. It suffices now to recognise that what a British archaeologist would casually call a hillfort or a fortified site has almost a dozen names in Germany. The implied historical explanations range from distant unoccupied hideaways to the quasi-city of princely leaders, from the communal effort of numerous independent farmers to the capricious whim of little kings surrounded by armed warriors. If the democratic vision of these fortifications is correct, then in fourth- to seventh-century Germania there were no nobles in the sense of medieval lords who exploited peasants demanding their labour or food renders or both. If such lords did exist, then it is likely that it was they who were responsible for the construction of early medieval *Burgen*. Either way, it is to the archaeological evidence that we must turn to in order to decide, evidence, I fear, that is sufficiently vague and equivocal that it allows debate about their historical meaning to continue.

Fortified Sites of the Fourth and Fifth Century

Gerhard Mildenerger (1978) compiled a gazetteer of German hillforts yielding finds from the Iron Age to the eighth century, thus effectively enlarging upon the few sites of this period included in von Uslar's (1964) work. The thesis of Hans Jürgen Brachman (1983) effectively covers the same material, although placed within an analytical, historical framework. The immediate strength of the latter is clear if only because it demands a more critical evaluation of the archaeological evidence. Thus Mildenerger's gazetteer is little more than a collection of find spots on hillforts and the tendency has been for subsequent archaeologists in the Federal Republic of Germany to add to the collection of possible early medieval *Burgen* with every new stray find (e.g. Wamser 1981; Abels 1983). One must question whether this evidence can be made to tell us anything. Mildenerger does not.

Even the date of some of the finds is very insecure and some sites appear in the gazetteer although there does not appear to be any evidence at all! Heiligenberg bei Heidelberg is mentioned because it has figured in others' writings although Mildenerger notes that there is no single scrap of evidence for

a fourth-fifth century date. Many of these sites do not figure in Mildenerger's own tables of possible dates of occupation because the occupation evidence is so very thin. Thus, Bonifatiusberg has yielded a spearhead which someone has claimed to be probably of Migration period, while Burgberg bei Königstein yielded a late Roman iron axe head. In the case of such simple iron tools or weapons, I do not feel convinced by such close dating. Seeburg could be discounted for it yields a single sherd which is 'thought' (by persons unknown) to be 'either late Roman or Migration period'. The inclusion of such dubious material is more detrimental than helpful. A single fibula is known from Gross Sarau dated to the late fifth or sixth century. It, however, seems best included with the large range of 'late Slavic' finds that come from the site and are interpreted as 'heirlooms', belonging to an extensive, but later occupation. Also we could exclude from the list Wittorferburg and Belau near the Baltic coast. Mildenerger does not make clear whether the numerous Roman pottery sherds are early or late, but more importantly, the *Abschnittswälle* which would separate the settlements, each on peninsulas, from a landward approach, have not been dated. In both cases there are further, later fortifications and settlements. It could easily be that the *Abschnittswälle* date to these later periods and that the settlement producing the Roman pottery was unfortified.

The implication should not be drawn that those sites which otherwise figure as possible *Burgen* reveal good evidence for occupation. Sulzburg, which figured in Werner's (1965) list and has been maintained by Mildenerger and Brachmann has yielded only a single fifth-century fibula. Ailenberg, Amöneburg, and Hammelburg yield only burials. Almost all the other possible sites reveal only stray finds, occasionally with pottery. How this material got to the site is an important but seldom-asked question. Middens or manuring with settlement refuse are unlikely alternatives, leaving occupation or grave-objects the most probable source. Thus Mildenerger follows the excavator's suggestion that the stray finds from Hasenburg derived from graves. Most of these sites have yielded few objects and seldom any that are inconsistent with human burial. Until an attempt is made to understand the depositional process there is little point in future generations of archaeologists adding to or altering the gazetteer, for as it stands it is historically meaningless.

In defence of the possibility of occupation we must admit that stray finds on the Runder Berg led to excavation and confirmation of settlement. The same will probably be true of Zähringer Burgberg. It is also striking that in contrast to the date range of finds within Trier, as discussed in chapter two, which exactly reflect those of *Reihengräber*, the date range of stray finds from *Burgen* do not. Thus

fourth and fifth century finds are more common than those of the sixth and seventh century. This represents an inversion of the frequency of objects from grave-assemblages as a whole, which is very striking, although the fact has not to my knowledge been recognised. Before we rush to the hasty conclusion that these stray hilltop finds do represent occupation, there is the complication that preferred cemetery locations of the fourth and fifth century are only poorly known, but that many small burial groups are found on hilltop sites and that such a site location for sixth- and seventh-century *Reihengräber* is very unusual. In other words, these stray hilltop finds may be due to the peculiar burial practice that we only dimly perceive. The only honest position is to say that the stray finds might as easily derive from non-settlement use of the hilltops.

Mildenberger's failure to consider *negative* evidence of occupation, on the other hand, is inexcusable. At Goldberg, although a few late Roman finds have come to light from excavation in comparison to the very numerous Iron Age finds, they scarcely seem convincing as evidence of occupation. In fact excavation has taken place at a number of Iron Age hillforts or of medieval castles: Alteburg bei Kassel, Babilonie, Bosenburg, Buraburg, Goldberg, Haynrode, Hohbeck, Meresburg, Pippinsburg bei Osterode, Quedlinburg, Staffelberg, Tilleda, and Turmberg bei Kasendorf. In all cases, however, the fourth- and fifth-century objects appeared as stray material without evidence of occupation. Even more striking is the absence of defensive construction. Hammelburg is regularly maintained to be a *Burg* on the strength of a possible, but by no means certain, historical reference. Yet there are no traces of any rampart on the hill. In some cases the opposite, so to say, is the case. At Amöneburg, Aschaffenburg, Meresburg, and Quedlinburg, subsequent building has obliterated any outwardly visible remains of fortification that may once have existed. There is, however, the hint that a Carolingian occupation layer underlay the walls at Amöneburg and the extensive excavations at Quedlinburg reveal early medieval occupation but the excavator was convinced there was no defensive enclosure. At Bosenburg and Pippinsburg bei Osterode, excavation of the ramparts revealed conclusively that no later - early medieval - refortification occurred. At Haynrode (Timpel 1975), Buraburg (Wand 1974), and Tilleda (Grimm 1958), there is conclusive archaeological evidence that no earlier - fourth-seventh century - fortification existed. Conceivably at Buraburg a much smaller enclosure of no more than a palisade *might* have existed, and left no visible rampart today. Such special pleading for the sake of one or two stray finds and a very large amount of negative evidence should be avoided. The suggestion that Tilleda might have been a fourth- or fifth-century *Burg* is totally unsupportable. The almost fully

excavated early medieval palace has yielded only a single late Roman fibula. There is no possibility that fourth-fifth century occupation occurred here, far less that it was fortified as well.

The gazetteer, as it stands and as it is frequently used by archaeologists (e.g. Weidemann, Schlesinger, Brachmann), contains a number of sites about which we can truthfully say nothing. The material debris found could derive from graves or ephemeral occupation and need have nothing to do with the visible rampart ruins that are in most cases probably Iron Age. As it is, the gazetteer also contains a number of sites that almost certainly were not occupied, fortified sites in the fourth and fifth century. There are, however, a few sites with good evidence for occupation, little as it is.

Glauberg (Budingen, Hessen). The fortification of *Glauberg* lies on a hill some five kilometres beyond the Roman *limes* near the *castellum* Altenstadt. Both overlooked the ancient route north through Oberhessen from Mainz to Fulda, *Glauberg* having a commanding view of almost 360 degrees. The site, almost a kilometre long, averaging 200 metres wide (thus with a two and a half kilometre circuit), is protected by an enclosure wall of various Iron Age constructions enclosing some 20 ha. (fig. 5.4). Both long sides are protected by a natural steep slope which falls away some 50 metres to the surrounding countryside. The east side, separating the fortification from the rest of the hill, is impressively protected by double walls and ditches still preserving a 13 metre difference in height from rampart crown to ditch bottom. The material for the wall came from inside the rampart forming a depression which runs around the site.

Abandoned in the first century AD, the site was reoccupied in the fourth century. On the rampart crown were found remains of a 1.5 m. wide drystone wall with large well-hewn blocks forming the exterior and interior face, between which was an earth- and stone-packed filling that was maintained by posts. Inside the wall were found sandstone plinths, which make one think of an access to a wallhead or a circuit path. Beyond the enclosure wall were two additional stretches of wall. On each of these were found similar drystone walling.

Occupation of the hilltop continued into later medieval times and the separation of different medieval phases is made difficult by the fact that the excavations occurred through the years 1933-39 and that the museum containing the finds was burnt in 1945 with most of the records; now only an interim report survives. From this report it would appear that large

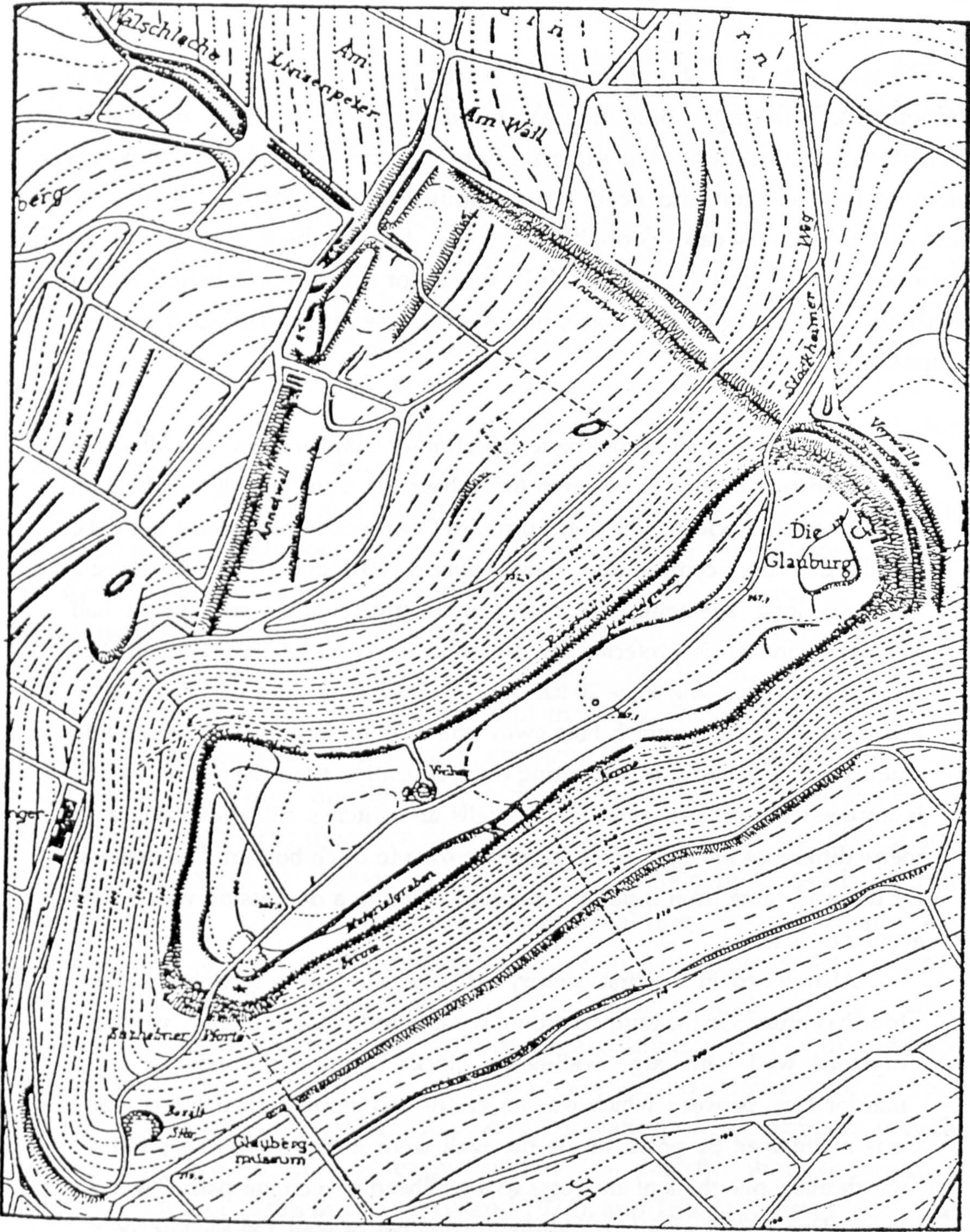


Fig. 5.3 Glauberg bei Büdingen (1:2000) (after Richter).

houses were found on the eastern part of the plateau. Smaller houses with stamped earth hearths lay in the depression of the ditch dug for rampart material and seemed to yield evidence of industrial production. Melting and casting pits were found and belt-buckle moulds. Eight moulds from the mid-fifth century came from one house. These artisans' houses had stone basements of dry stone walling and were set only 0.2 to 0.5 metres from one another.

The finds seem to date from the second half of the third century but the majority are from the fourth and first half of the fifth. None date later than AD 500 (Werner 1965), but the excavator nevertheless claimed there was a Frankish 'castle' of the late seventh to ninth century in the restricted area behind the south gate. The foundations of later medieval buildings abound. Small-scale excavation presently being carried out by the Landesamt für Denkmalpflege Hessen may hopefully throw more light on the constructions within the fortification.

Bibliography: Richter 1934, Werner 1965, Fehring 1972a and 1972b.

Runder Berg (Urach, Reutlingen, Baden-Württemberg). The hill Runder Berg overlooks the town Urach on the northern edge of the Swabian Alb, dominating the Erms valley as it narrows to little more than a gorge. It lies just within the Roman *limes* that was broken in 260/1 and was never to be re-established. The very few objects dating to the first and second centuries were most likely very old when they arrived on the site. From the second half of the third century the number of finds dramatically increases, material of various kinds is well represented from the fourth and fifth centuries, and the finds spectrum ends abruptly in the first quarter of the sixth. The material comes equally from the hilltop and the various terraced areas. Sadly, the very thin soil cover means that almost all previous occupation layers have disappeared and what remains of structural evidence is largely restricted to stone-cut features. To this interesting period only two constructions can be definitely assigned. In the north-eastern corner a ditch some 30 cm. wide and cut some 50-60 cm. into the rock has been discovered. Only a few prehistoric finds come from the fill, but it very probably dates to the early Alamannic occupation, and is interpreted as having supported a wooden palisade. More certainly of this period, and post-dating the thin rock-cut ditch, is the enclosure consisting of paired posts, averaging half a metre in diameter, lying 3 metres apart and sunk just over half a metre into the natural rock. Just how this

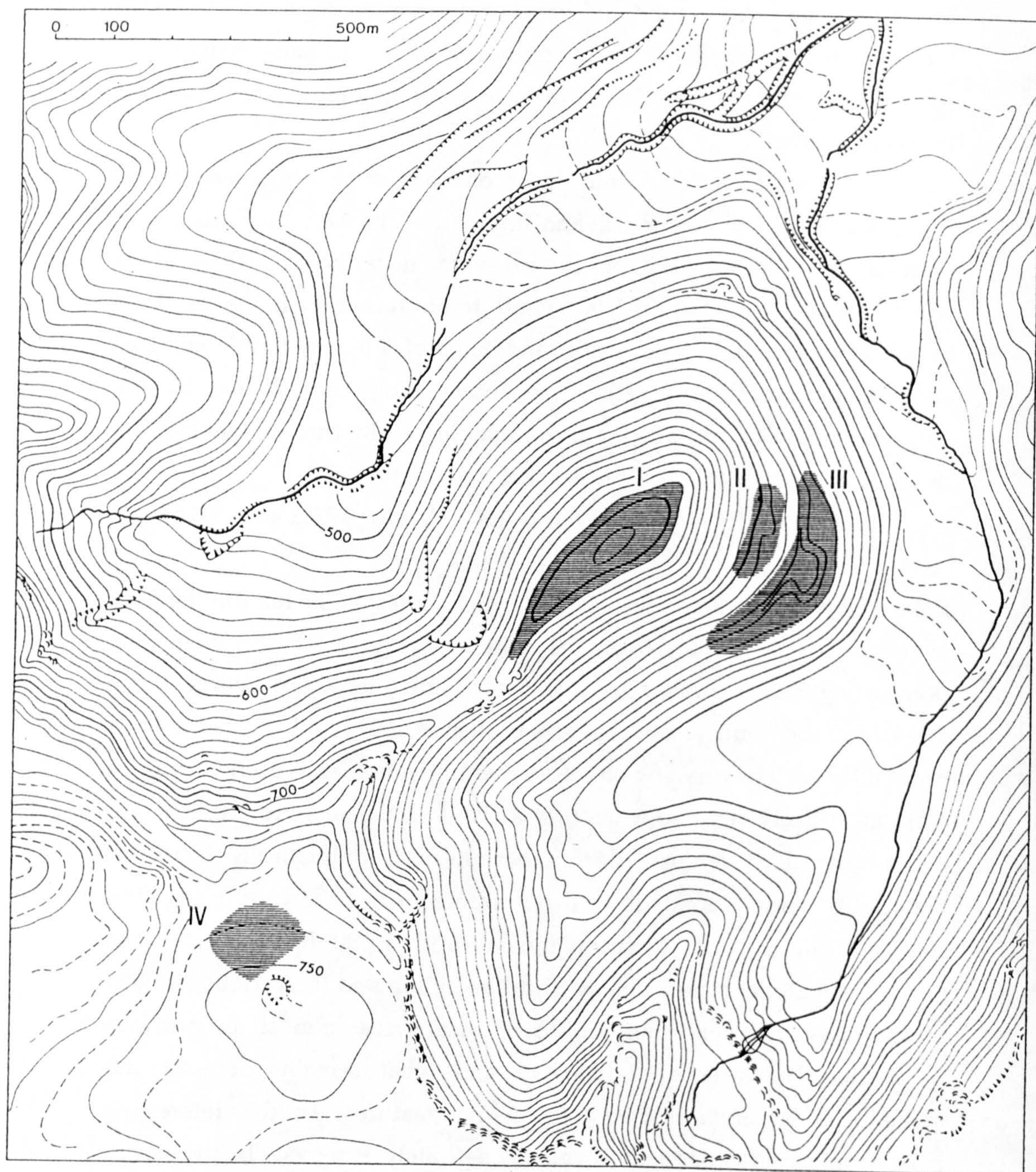


Fig. 5.4a and b Runder Berg bei Urach. The shaded areas show where early medieval material has been found. Facing: the post-holes forming part of the early rampart is shaded, as are later houses and a short section of the later, lower stone rampart. The great timber building of the early period is marked out in red (after Milojcic).



enclosure could be visualised in reconstruction is disputed, for a certain amount of stone is found on the line of the circuit, although most agree to seeing it as a purely timber construction.

Spread across the whole site is the evidence for production of a variety of objects. Carpentry, leather working, comb manufacturing, jet ornament production, smithing, and precious metal working have all been documented. Even glass working has been detected, but more astonishing than simply the raw glass was the discovery of a glass-blowing pipe!

Although the excavator, Vladimir Milošević, despaired of ever sorting out the tangle of workshop activities, Ursula Koch (1984) has shown that, indeed, the different types of production were carried out in their own separate work areas, but her most important conclusion was that in the fifth century the workshops stopped working in the north-eastern side of the hill. In short, production activities were removed to outside the double-posted enclosure.

Bibliography: Koch 1982, 1984, 1987, Milošević 1975, 1979.

Gelbe Burg (Dittenheim, Kr. Weissenburg-Gunzenhausen, Bayern). *Gelbe Burg* consists of two concentric enclosures (fig. 5.5). The inner plateau is approximately 6 ha. with sides 225 x 275 m. in length, enclosed by a prehistoric rampart. Some 30 metres lower vertically a second rampart, roughly triangular in plan, encloses 20 ha. Two sections of this lower enclosure revealed a 13.3 m. wide rampart composed of a drystone front and rear facade that contained a stone and earthen core. A late fourth-century glass sherd was held to date the construction. Cut into this rampart was a later stone wall, which was poorly preserved and thought to date probably to the tenth century.

Numerous finds of the fourth and fifth century have come to light from the upper plateau as the result of ancient excavations, which also revealed a two-phased rampart. The earlier of the two was clearly of prehistoric date, the later, a 3 m. wide limestone wall, was undatable.

Bibliography: F.-R. Herrmann 1969-70; 1970.

Staffelberg (Staffelstein, Kr. Lichtenfels, Bayern). *Staffelberg* consists of an upper plateau, 350 x 125 m., and a lower plateau, 900 x 700 m. A massive rampart separates the site from the connecting upland while the edges of the plateau were only weakly defended. The fortifications, however, date to various periods of the Iron Age and late Roman Iron Age defences have

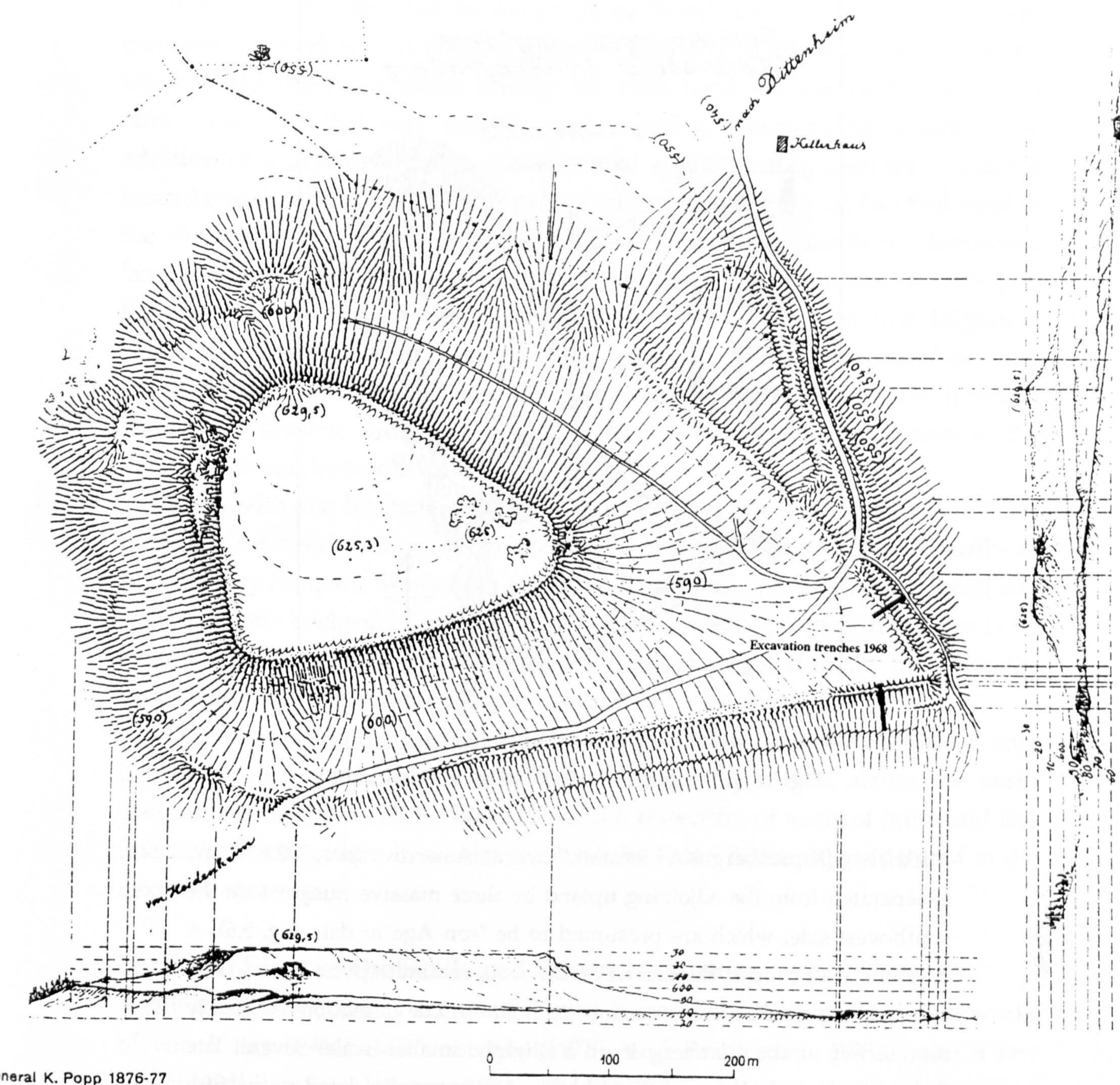


Fig. 5.5 Gelbe Burg showing two excavation trenches (1968) where early medieval material was found in the rampart (after K. Popp).

Bibliography: Mildenberger 1978.

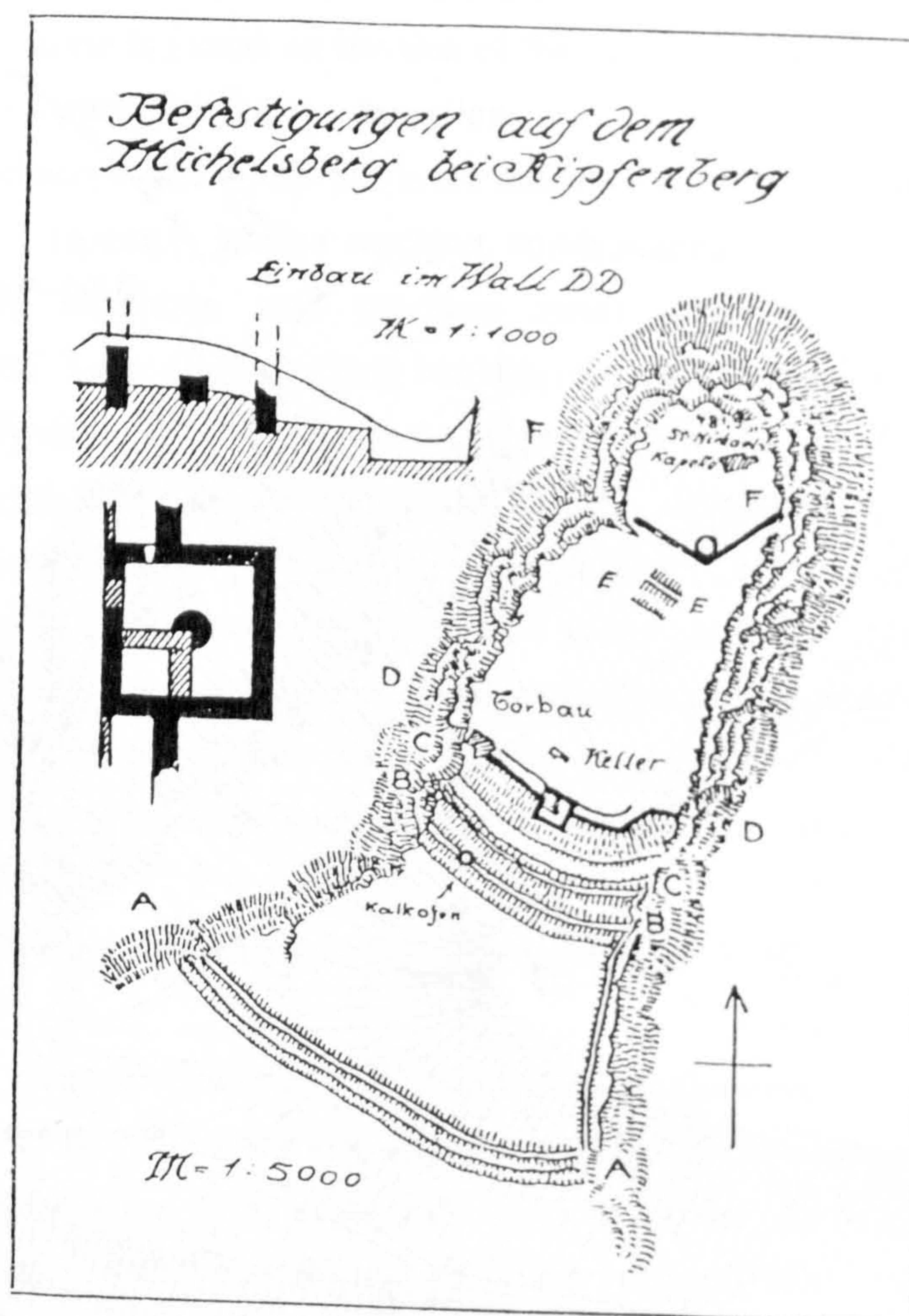


Fig. 5.6 Michelsberg (1:5000) (after Winkelmann).

Michelsberg (Kipfenberg, Kr. Eichstatt, Bayern). A narrow spur, 200 x 100 m., is separated from the adjoining upland by three massive ramparts on the south-west side, which are presumed to be Iron Age in date (fig. 5.6). A drystone wall capped the crown of the wall, reminding von Uslar (1964, 162) of late Roman construction technique. The situation is clearly reminiscent of the Glauberg if on a slightly smaller scale. Several late Roman finds, including well-fired black pottery usually dated to the fifth century, come from the interior. At the foot of the hill a *Reihengräber* cemetery has been excavated, dated to the sixth and seventh centuries, but also yielding a few fourth and fifth century finds.

Bibliography: Von Uslar 1964.

To these can be added the Zähringer Burgberg and a promontory fort with a rampart of a drystone wall front and earth-and-timber backing in a loop of the Main at Urphar (Markt Kreuzwertheim, Kr. Main-Spessart, Bayern) (Wamser

1981). More cannot be said until publications appear.

It is not an accident that the *Burgen* of the fourth and fifth century are almost exclusively found within re-used hillforts. Iron Age hillforts already occupied many of the most favourable settings for such large enclosures. Moreover, on offer were sites that only needed, in the case of Glauberg for example, the addition of a drystone wall to the crown of a pre-existing rampart to make a formidable defensive circuit. But perhaps more important is the fact that most of the evidence for fourth- and fifth-century occupation has been discovered 'accidentally' during excavation of Iron Age hillforts. In comparison with the pre-Roman Iron Age, the number of sites for the late Roman Iron Age/Migration period is both small and includes an unusually high proportion of very questionable sites. Occupied sites of this period, being by their nature, it would seem, very elusive, provide a biased picture of hillfort re-use because of the intensive research centred on them.

Even with the limited amount of good evidence we can say that hillforts present a far from uniform picture. Staffelberg possibly never had a fourth- or fifth-century rampart; Runder Berg was small and had only a relatively small and weak palisade enclosure (and in this case it is interesting to note that there is no evidence for the existence of any previous hillfort here); and finally there was the very large Glauberg and the massive rampart of Gelbe Burg.

The distribution map would appear to suggest that *Burgen* were known only in middle and southern Germany and were largely absent across the north German plain. The result is certainly not the reflection of areas of more and less intensive investigation; it is quite real and the same distribution is found in the next two centuries.

Fortified Sites of the Sixth and Seventh Century

Again the list of possibly occupied *Burgen* comes from the gazetteers and works of von Uslar (1964), Mildemberger (1978), and Brachmann (1983) with a few recent additions (e.g. Wamser 1984). The list of possible sites is even shorter than that of the two previous centuries and includes candidates even more dubious than those of the earlier list.

Again sites figure in Mildemberger's list that yield only the poorest of evidence. Altenberg bei Canstatt, Mildemberger himself notes, is frequently considered a Merovingian site but without documentary or archaeological evidence. The early medieval dating of this hillfort in the neighbourhood of Canstatt is presumably the result of association with the famous Bloodbath of Canstatt, the battle restoring Alamannia to the hegemony of the early

be argued that criteria for acceptance should be made even more rigorous, for without indisputable examples dating before the mid-seventh century to provide us with analogies we should perhaps be more wary of the evidence of stray finds. This is perhaps strengthened by the observation that material of the sixth and seventh centuries is generally much more common than that of the fourth and fifth because of the different burial practices.

Konrad Weidemann (1975) puts forward fourteen *Burgen* in Hessen and Mainfranken which he believes were occupied in the seventh century. Only some of these figure in Mildenerger's list (e.g. Glauberg, Würzburg, and Hasenburg) despite Mildenerger's excessive zeal in claiming possible candidates. Weidemann argues that sites which yield evidence of occupation in the first half of the eighth century probably were already occupied in the seventh. The suggestion is plausible enough, but many of the sites suggested yield little enough evidence and there is no particularly good reason for trying to date them to a period before which any datable finds at all are known. Weidemann also includes sites such as Christenberg and Büraburg, although the excavators in both cases prefer to date the earliest phases to ca. AD 700. Thus, as Schwind (1974, 216) notes, Weidemann is the only person to have suggested an earlier date. Exactly why he is so anxious to produce a series of seventh-century *Burgen* is not clear, but in a later chapter it will be seen that Weidemann's suggestion has even less to recommend itself.

The only site of this period yielding sufficiently good evidence to warrant discussion is the Runder Berg.

Runder Berg (Urach, Kr. Reutlingen, Baden-Württemberg). The demise of the first Alamannic settlement on the Runder Berg has been described at the beginning of this chapter. The site was reoccupied one and a half centuries after abandonment, in the latter half of the seventh century. An area greater than before was then enclosed, about half a hectare, and this time with a stone wall of varying thickness and construction. This implied to the excavator that it was repeatedly repaired, an idea made more attractive as the wall looks to have been rather unstable and no special arrangements were made for the foundations. Behind the enclosure wall stone-built houses were found, which can be divided into two categories: one built with a small gap (0.3 m.) between the houses and enclosure wall, and one built with the enclosure wall as their rear wall. In the former case the enclosure wall itself measured some 2 m. in width and was built of large natural blocks and slabs with a limeless clay mortar. In the latter case



Fig. 5.7 Runder Berg, one of the earlier stone buildings (after Milošević).

the enclosure wall was only 1.4 m. wide, less carefully built of small stones bound with earth and tuff fragments giving the impression that the wall was built quickly and perhaps to span a breach in the wall. No ditch lay before the wall, but on three sides access to the site is very steep. The buildings of the two groups have further distinguishing characteristics. The former group were larger, about 8-9 x 5-6 metres, the latter 5 x 4 m. The larger houses had terraced floors that were carefully cut into the bedrock and walls 0.8 m. thick made of carefully chosen slabs, again bound in a clay mortar. Burnt evidence of three rows of three posts along the walls and on

the rock-cut floor of at least one building is interpreted as holding an upper floor. As the base of the buildings was often three metres below the plateau surface, it was believed that the wooden first floor gave access directly to the hilltop. The smaller houses were in every way inferior: of less carefully chosen stone, without binding material, and with less carefully levelled floors. The two groups are thought to have been of different phases of occupation for yellow-white upper Rhine wheel-thrown pottery, a local variation of the middle Rhine Badorfer ware, was commonly found in the collapsed debris and fill of the larger houses, but often directly on the floors of the smaller. Further finds put the use of the smaller houses in the late seventh or early eighth century. Elsewhere on the site there were two or three small stone built chambers with interiors that were severely reddened by fire, which was interpreted as caused by ovens, perhaps baking ovens.

The summit of the hill seems to have been covered predominantly by wooden constructions; all the stone buildings were built on the slopes. This is considered all the more peculiar as so little soil cover existed on the hilltop. A couple of *Grubenhäuser* have been recognised, but in the main no sense can be made of the array of post-holes. Milošević stressed that the wooden constructions were important, although the details of most such constructions escape us. Not so in the case of a single 20 x 9 m., three-aisled, post-built construction. The latest dated object from the post-holes was a strap-end dated to around AD 700. The building is thus thought to have been constructed early in the eighth century.

Finds from this second occupation do not show any of the intensive industrial production of the first. They have an entirely domestic aspect: dress fasteners and ornaments, keys and locks, and a surprising number of items relating to horse riding, including harness and saddle fittings. The dating of the finds seems to extend into the ninth and tenth centuries although in ever decreasing numbers.

Bibliography: Koch 1982, 1984, 1987, Milošević 1975, 1979.

Burgen and Lords: the Historiographic Background

Research into hillfort occupation during the Roman imperial and early medieval periods could be said to have begun with Carl Schuchhardt (Schuchhardt and Opperman 1888-1916). It may be remembered from above that Schuchhardt used the term *Volksburg*, but not in the Marxist sense of a fort of the people, by the people, and for the people. For Schuchhardt it was the product and residence of

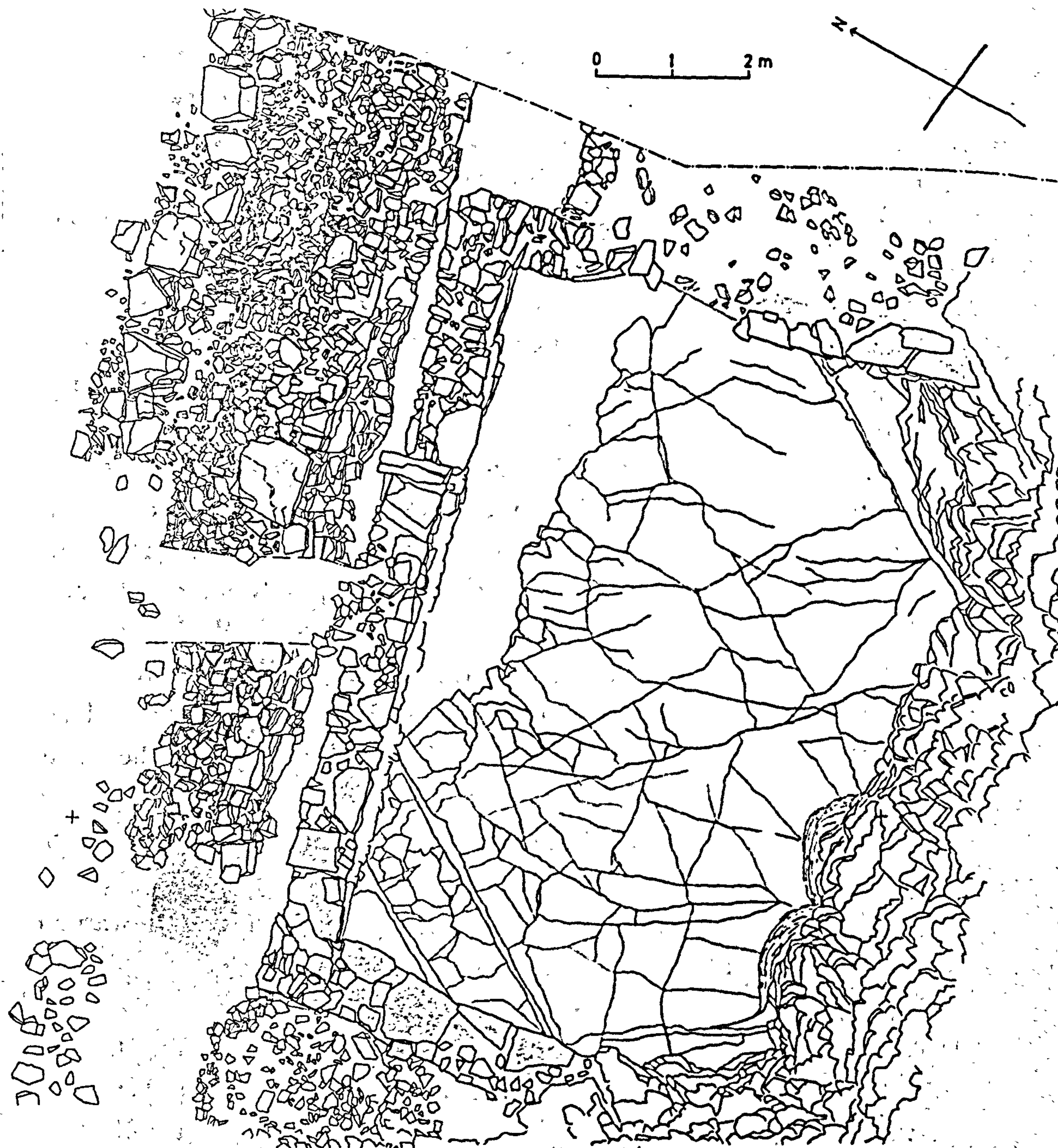


Fig. 5.7 Runder Berg, one of the earlier stone buildings (after Milošević).

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nobles with whom the people took refuge in times of danger. The emphasis on communal or tribal life, however, was sufficiently strong that Dannenbauer felt justified in portraying himself as a heretic when he offered a new interpretation. The publication of his essay '*Adel, Burg und Herrschaft bei den Germanen*' in 1941 marked the beginning of a historical debate that still dominates much modern research (Fehring 1987, 92). Dannenbauer's central theme was that from the earliest documentary sources, those of Caesar and Tacitus, to the effectual beginning of German medieval history, the Ottonian period, there existed nobles exercising a type of lordship which was similar in form to that of the central Middle Ages. In short, he argued for the existence of lords who owned vast tracts of land, exploited the agricultural production of peasants, maintained a number of dependent followers who formed a small private army, and dominated both countryside and country folk from their lordly *Burg*. One of Dannenbauer's aims was to refute the 'Germanic school' which portrayed the Germanic peoples of the Roman and early medieval period as democratic free warrior peasants.¹

Central to his proof refuting these positions were the literary and documentary sources of Caesar, Tacitus, Ammianus Marcellinus, Widukind, and various Carolingian documents concerning Charlemagne's wars to the east of the Rhine and missionary work among the Germans. Finally Dannenbauer used the early monastic cartularies of St. Gallen and Lorsch. Estates with large numbers of dependent serfs and slaves were hardly donated to St. Gallen by anyone other than nobles owning vast tracts of land. Dannenbauer's sources are in fact almost all the documentary evidence for the trans-Rhenan Germans before the reigns of Louis the Pious' sons. The paucity of the evidence meant that for each of the three areas Dannenbauer examined, namely Saxony, Thuringia, and Alamannia, there were inevitable lacunae which forced him to turn to the archaeological evidence.

The Hassleben-Leuna graves of the early Empire, the Lubow graves of the later Empire, and the various rich graves of the *Reihengräber* are all widely accepted as proof of the existence of German *Fürsten*, the lordlings on the right bank of the Rhine whom Dannenbauer saw in the documentary sources and the

¹ Dannenbauer is mainly credited with this founder's position in the debate about the origins of lordship and fortifications because he expressly talked in terms of hillforts and archaeology. However, Dannenbauer's claim to be a rebel was exaggerated. Dopsch had long been arguing similarly against a picture of lordless, egalitarian early Germanic society, and with considerably more erudition. Indeed, such a communist, communalist, 'democratic', egalitarian view of Germanic society had hardly been fashionable at the start of the Third Reich, far less in 1941. Richter, the excavator of Glauberg throughout the 1930s, had interpreted the site in a similar way. Indeed, Dannenbauer more or less incorporates his interpretations into the essay wholesale. Interestingly, Richter is photographed in his interim report in Nazi uniform; Dannenbauer's supposedly heretical approach had long been supported by Fascist-sympathising historians.

supposed builders of the archaeologically-revealed *Burgen*.

Fifteen years later, when Dannenbauer's article was republished, his arguments had become entrenched opinion. In particular, W. Schlesinger had published a series of articles accepting Dannenbauer's premise of continued hillfort occupation and its connection with early medieval princes. It was Schlesinger's particular position that lords with their followers formed a major part of Germanic society continuously from Roman times until the full blossoming of the Middle Ages. Of similar persuasion, another historian, R. Wenskus (1961), accepted the notion as self-evident.

Mildenberger (1978) has defended Dannenbauer's position, although archaeologists were never so completely convinced as were the historians. Their scepticism, however, is largely to be judged by their silence. In Mildenberger's (1978) own introduction to this historiography, he only just stops short of criticising archaeologists of the immediately pre- and post-war years for their lack of interest in 'German' *Burgen*, finding interest rather in Bronze Age, Hallstatt, La Tène, and later medieval castles. In several regional gazetteers, Mildenberger notes, Roman and post-Roman sites are conspicuous by their absence. It is no wonder, he says, that when the first major gazetteer of early medieval fortified sites was attempted, by von Uslar (1958), it really only began with the Carolingian period. Mildenberger, however, misrepresents the work of these archaeologists. It was not the lack of interest that left the first seven centuries AD unnoticed and unpublished, but the lack of *evidence* for occupation.

From the publication of von Uslar's *Studien zu frühgeschichtlichen Befestigungen* in 1964 onwards, a renewed interest in the problem of Roman and immediately post-Roman fortified settlements can be detected. The general opinion, however, has been that they may not have existed, or more correctly, that the archaeological proof of their occupation is lacking. Thus in the words of von Uslar (1964, 14), 'in any case, for the following period [from the birth of Christ] until the third century - as far as I can see - archaeological proof is entirely absent,' and further (1964, 31) 'fortified sites of the Migration period in the territory of the former *Germania libera*, have, with the exception of Glauberg in Oberhessen, astonishingly not yet been certainly proven.' Von Uslar's position is clear: from the first to seventh century there exists incontrovertible evidence of only one hillfort site being occupied in the territories beyond the Roman *limes*, but that in the post-Roman period such occupation was clearly to be expected.

The situation changed, as von Uslar (1964, 34) noted, in the eighth century. The fortifications which appear in the records of Charlemagne's German wars are equally apparent in the archaeological record. For this reason Mildenberger, who

sets himself the task of proving the continuity of Germanic *Burgen*, decides to end before the Carolingian period: their existence by then is indisputable. Dannenbauer had accepted continuity by leaving a somewhat hazy chronology. Tacitus, so Dannenbauer believed, proved first century evidence, while Carolingian occupation was clear from documentary and archaeological evidence, so that a few archaeological hints of something in between, like the Glauberg, were all that Dannenbauer needed to prove his case. Joachim Werner (1965) was more rigorous. He denied completely the occupation of hillforts during the early empire and claimed that after 500 in Alamannia, hillforts were abandoned, so that Merovingian Frankish or Alamannic nobles, like the *principes* of the early and middle empire, grounded their lordship 'without *Burgen*' and 'solely on estate possession and dependency.' By examining the period more minutely, Werner was able to contradict Dannenbauer's claim of continuous usage of fortified sites.

Although we have seen that subsequent research possibly disproves Werner's position on the Merovingian period, it is still the accepted general position that hillfort occupation cannot be proven during the early empire. I have quoted von Uslar holding this opinion in 1964. Werner repeated it in 1965. Ten years later Malcolm Todd (1975) likewise denied hillfort occupation, at least during the second and third centuries. The position was similarly held by Edith Wightman (1985), and yet with substantially identical material at hand Mildemberger (1978) suggests continued Germanic occupation of hillforts from the late La Tène Iron Age to the Ottonian era. How can this be? Because of the paucity of extensive excavations as at Runder Berg (which yields definite evidence of Dark Age occupation) Mildemberger's work necessarily depends on a method of simply collecting all reported finds of Roman or post-Roman material from hillforts. The full implications of the presence of such material probably cannot be understood, but Mildemberger's assumption that it testifies to both occupation and fortification is surely too premature. It would be wrong to imply that Mildemberger was doing anything new in practice; Werner (1965) recognised that with one exception 'in all other cases it is a question of settlement evidence from the fourth and fifth century resting on more or less numerous stray finds.' Werner himself based his findings largely on the synthesis of R. Roeren (1960) who similarly catalogued find spots, but who also recognised the intrinsic weakness of the evidence for allowing assumptions to be made about occupation. Whether stray finds represent evidence of permanent settlement, disturbed burials, or simply a picnic remains unanswerable without the nature of contemporary settlements being more fully understood. Although there remains

a vast scope for collecting new information about fourth- and fifth-century hillfort occupation, we are not completely in the dark. When Werner wrote, it should be noted, the Runder Berg bei Urach was only a possible site, although admittedly the quantity of finds that had come from the site was considerable. Now, following extensive excavation, it is one of the few incontrovertible sites. Although not extensively excavated, Zähringer Burgberg has similarly moved from the possible category on the strength of scattered finds, to the definite category following excavation, as did the Gelbe Burg, albeit less convincingly. Thus it is that, with the addition of Runder Berg, Gelbe Burg, and Zähringer Burgberg to the certain category of occupied hillforts, so our belief that other stray finds of the fifth century may represent evidence of permanent occupation is strengthened.

But why should it be that Mildenberger accepts these stray finds as indicating probable occupation so readily and so uncritically? The answer, I suggest, lies in the title of his book, *Germanische Burgen*. It takes Mildenberger more than twelve pages of an otherwise very concisely written book to explain the chronological and geographical limits of his investigation, in the course of which Kossinna's name makes numerous appearances. Mildenberger is forced to make this exceptionally detailed justification of his subject, because he has chosen to investigate only 'Germanic' hillforts. He is forced then to assign ethnic tags to his archaeological evidence; this is the reason for Kossinna's recurring appearance. Rightly Mildenberger avoids seeing various archaeological phenomena as deriving from external sources when it can be plausibly suggested that they were native developments, but the sentiment seems to be that the German *Geist* did not need these foreign ideas, that the knowledge of hillfort construction and occupation was an ingredient of the Germanic personality. When considering the Merovingian period east and west of the Rhine in areas of the former Roman empire, Mildenberger decides to include the former because there was 'no notable continuity of population', by which he means that the Romans were annihilated and replaced by 'real' Germans, while the territory to the west is excluded from consideration, for the fortifications controlled by the Franks 'are to be seen as late Roman rather than German'. This example puts Mildenberger's justifications in an even less sympathetic light than that of the German *Geist* I imputed, for here the Germanic quality is to be found within the inanimate constructions themselves. In the pages of Mildenberger's book, German *Burgen* seem to survive on German soil at times like endangered species reduced to a handful of breeding pairs, and like endangered species there is a feeling that if the *Burgen* should die out at any time, reappearance would necessarily be the

result of reintroduction from abroad. Thus chronological gaps must be filled to keep the hillforts alive and to keep them 'German'.

I would argue that Mildenerger's *modus operandi* is historically wrong and that by discarding his uncritical attempt to prove the continuous 'German knowledge' of the *Burg* we find two historical questions emerge that would otherwise have languished under the blanket of 'continuity'. By removing Mildenerger's opposition to the nearly universally held view that the centuries preceding the fourth did not see any general occupation of hillforts, the interesting question arises, Why should hillforts reappear in the fourth and fifth century? Unlike Britain or north-eastern Gaul, no vague arguments such as a return to Iron Age conditions and traditions can be put forward for Germany east of the Rhine.

Historical Background

That the sites discussed above are found primarily in south-western Germany is very suggestive, for Alamannia deserves special treatment among the Germanic regions east of the Rhine. Unique among the Germanic barbarians, the Alamanni conquered and occupied Roman territory, the *agri decumates*, in the third century. Numerous incursions during the third century preceded the invasion of 259/60 which wrested the territory permanently from Roman control. Wars on all fronts and a series of usurpations ensured that any Roman attempts to reconquer the territory were long postponed. From Probus onwards Roman incursions into Alamannia never seriously threatened Alamannic control and were aimed more at laying waste the land in retaliation for Alamannic raids over the frontier.

Werner's (1965) essay on Alamannic fortifications argues for a fourth- and fifth-century florescence of *Burgen* preceded and followed by a couple of centuries without fortifications. Werner saw the cause for the building of these defences in the conflict between the Alamanni and the Roman army following the conquest of the *agri decumates*. With the collapse of the Roman empire the need for these fortifications ceased. Or rather, to portray Werner more accurately, with the incorporation of Alamannic territory in the Merovingian empire the last external military threat disappeared and therefore too the need for the *Hohensiedlungen*.

It was noted by Dannenbauer and Werner, among others (perhaps first was H. Richter (1934)) that in Ammianus' work, *Rerum gestarum libri*, there are references to the Alamanni fleeing from the Roman army into the hills (27.9; 27.10; 31.10; 31.12) or drawing up their soldiers in readiness for battle on a hilltop (17.1; 27.9; 27.10): '*montem . . . per cofragosos colles undique praeruptum et invium absque septentrionali latere.*' The natural tendency is to see hillforts behind these

references. All three archaeologists put forward the suggestion when only a single definite archaeological example was known, the Glauberg. Surprisingly few have made use of the *Ravenna Cosmography* as supportive proof of hillfort occupation, although it is occasionally mentioned in passing. Thus Ascapa, Ascis, Rizinis, and Uburzis of the *Ravenna Cosmography* have been interpreted as representing Aschaffenburg, Hohenasperg, Reissensburg, and Würzburg. Perhaps the *Ravenna Cosmography* has been largely neglected because of the difficulty in identifying sites. Many names, such as Turigoberga, have been postulated as representing various sites, for instance Untertürkheim bei Stuttgart or Durreberg bei Tübingen (Dannenbauer 1941, 39), or no site has been postulated at all. Even when there is some agreement about the site, with the single exception of Würzburg, none have revealed a shred of archaeological evidence for fourth- or fifth-century occupation. Of course, one important conclusion can be drawn from the *Cosmography*, that is that the Romans recognised several centres as being particularly important within Alamannic territory and presumably these were centres of political importance or centres for trade or both. Such centres are not recognised by the *Ravenna Cosmography* north of the Main.

That a degree of *romanitas* lingered in the *agri decumates*, even two centuries after the Alamannic invasion of 259/60, is perhaps suggested by the subjugation of Alamannia to the Merovingian kingdoms. Whatever the so-called 'Germanic' elements of Merovingian society and culture, the essentially Roman basis of property ownership, dependency, and authority on which the Merovingian kingdoms were built is clear from the boundaries that defined these barbarian kingdoms. The land beyond the Rhine, the same land which lay beyond the Roman *limes*, is portrayed repeatedly by Frankish sources as pagan, savage, and beyond the permanent control of the civilised Merovingian kingdoms. Again, therefore, the fact that the Merovingians found the region east of the Rhine but south of the Main more malleable to their rule, suggests that the Alamanni too had been influenced by the Roman society they conquered.

It would not seem an unreasonable starting hypothesis that Alamannia would reveal continuity from Roman settlements, that the villa organisation might have survived, albeit in reduced and modified form, as has been suggested for Gaul.

Ammianus Marcellinus (17.1, 7) gives us immediate hope that just such continuity is feasible when he noted that after the battle of Strasburg (346 AD):

the Romans plundered farms rich in cattle and crops . . . and set fire to and burned down all the houses, which were built quite carefully in Roman

fashion (*opulentas pecore villas et frugibus . . . domicilia curatius ritu Romano constructa*).

As if to prove Ammianus right, the site of Ebel bei Praunheim, near Frankfurt, has been cited as fitting the description (Woelke 1937; Werner 1965). Here a Roman *villa rustica* almost certainly survived beyond the late third century. There were traces of wooden extensions added to the stone dwelling which appear to be datable by 'Alamannic' finds to the fourth or fifth century. It must be admitted that the additional building was of considerably less substantial quality than the Roman bulk of the villa.

It should be noted that the buildings Ammianus saw and the villa of Ebel were immediately across the Rhine near Mainz and not deep in the Alamannic hinterland. Closer to a major concentration of Roman occupation this corner of Alamannia could not have been. This area was perhaps exceptional in the survival of Roman culture into the fourth century. There is otherwise a general lack of evidence for continued villa survival and many stress a complete break. 'There is no doubt that the Roman villa culture, that is the geographic settlement pattern of individual *villae rusticae*, found its demise as the result of the third century wars,' for 'in Baden-Württemberg there is not a single villa known to have been occupied after this time,' notes Gerhard Fingerlin (1974, 77 and 48). Fingerlin also notes the common occurrence of burning in the final layers of villas in Baden-Württemberg. This is reminiscent of the fire and slaughter explanation of Anglo-Saxon and Frankish settlement in Britannia and Gaul. Weidemann (1972) has shown that activity of some description must have continued on a large number of Roman *castella* after the loss of the *agri decumates*, and he points to several villas as well. He presents a substantial list of sites from which Roman material that post-dates 260 has been found. Interestingly, coins are often found on these sites. What sort of occupation is implied by these finds has yet to be considered by archaeologists. Some Alamannic finds can be found on *villa rusticae* and even a *Grubenhaus* is known from Ladenburg (Schallmayer 1986) (fig. 4.2). What makes the Alamannic region so difficult to interpret is the almost complete absence of fourth- and fifth-century evidence. Thus while *Reihengräber* allow a relationship between fourth-century villas and fifth-century settlement in Gaul to be postulated, in the Alamannic regions the relationship between settlements and cemeteries cannot be recognised. It is clear, however, that the relationship of Roman villas to medieval churches and village sites, which is visible in France and Switzerland, is absent in Baden-Württemberg; there are no Badische villages that can be suggested as descendants of Germano-Roman villa settlements. This is not surprising, however, given that there were at least four hundred years

between the first timber churches built in Alamannia and the last Roman villas built in the *agri decumates*.

There are other reasons, beyond simple destruction during the Alamannic invasion, for believing that continuity of Roman villas is unlikely. It should be remembered that the conquest coincided with the late third century crisis – whether or not it was itself triggered by barbarian incursions – which left some half of the northern Gallic villas abandoned or destroyed. Outside the Roman empire there would clearly not have been the economic conditions, often seen in the form of production for an insatiable Roman army, to revitalise the villa estates. That Alamannia was not simply beyond the Roman *limes* in the fourth century, but was also treated as the home of a particularly dangerous people and was repeatedly laid waste by the Romans, must have further decreased the ability for economic recovery.

The late third, fourth, and fifth centuries are frequently treated separately from the subsequent centuries in Alamannic history. Partially this is because the two periods are separated politically, the former dominated by numerous Alamannic princes and the latter by Merovingian *duces*. One suspects, however, that the differential conditions of archaeological survival play a part. Before the sixth-century appearance of *Reihengräber*, the archaeological collection is meagre. Although archaeologists and historians consciously recognise that between 260 and 500 AD Baden-Württemberg was not devoid of inhabitants even if the evidence for their existence is not forthcoming, subconsciously they are certainly troubled by this absence and interpret it as meaning that the land was far from fully stocked. Thus Geuenich (1982) refers to this period as the first phase of the Alamannic *Landnahme* and Fingerlin (1974) suggests that migration into the *agri decumates* was continuous after the penetration of the *limes* until the appearance of the *Reihengräber*. The most revealing of all instances comes during Christlein's (1979, 29) discussion of the two alternative views of the settlement story that he can picture, following the first appearance of *Reihengräber* cemeteries at the end of the fifth century. In one hypothetical reconstruction a single family settles down, in the other several families 'which had united *before* settling down in the new homeland' (Christlein's own emphasis). In a chapter which began by quoting all the evidence that pointed to the Alamanni being a numerous people in the late third and fourth century, Christlein had managed after only three pages to slip unconsciously into a position that regarded *Reihengräber* as the remains of new immigrants and immigrants to an empty country.

I am not sympathetic towards these interpretations. There is little, if any, evidence for continued migration into the *agri decumates* after the documented

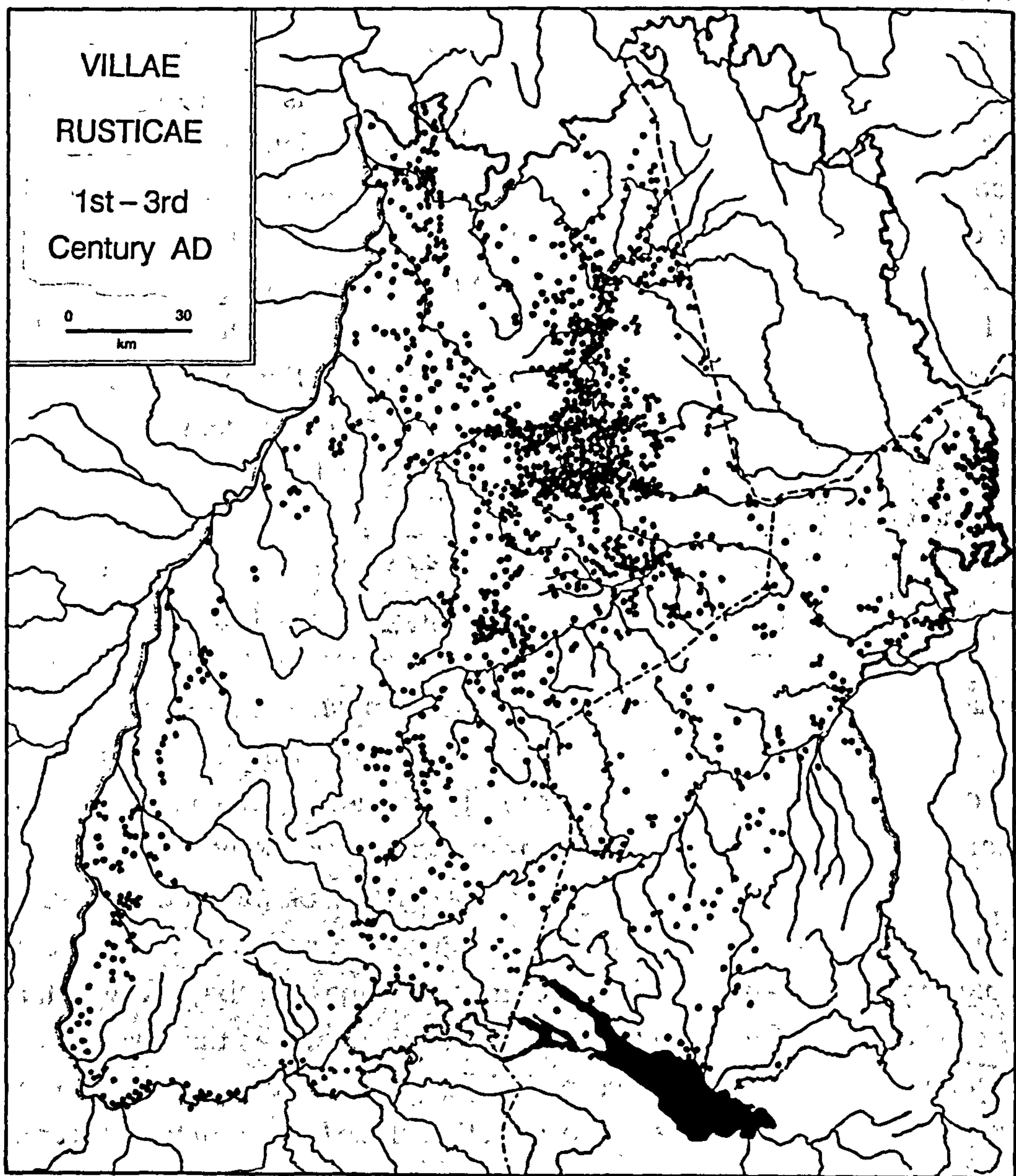


Fig. 5.8 Distribution of villas in the agri decumates (after Planck).

conquest by the Alamanni. On the other hand there is the evidence of later political conditions which reveal the Alamannic territory to have extended beyond its fourth- and fifth-century boundary along the Rhine into Alsace and northern Switzerland. This is more plausibly associated with the movement of people out of the old *agri decumates*, for with the extension of the political boundaries came also (and very quickly?) the extension of the German languages. One might prefer to see the population of Baden-Württemberg between 200 and 500 AD increase slightly from natural growth and the arrival of new immigrants,

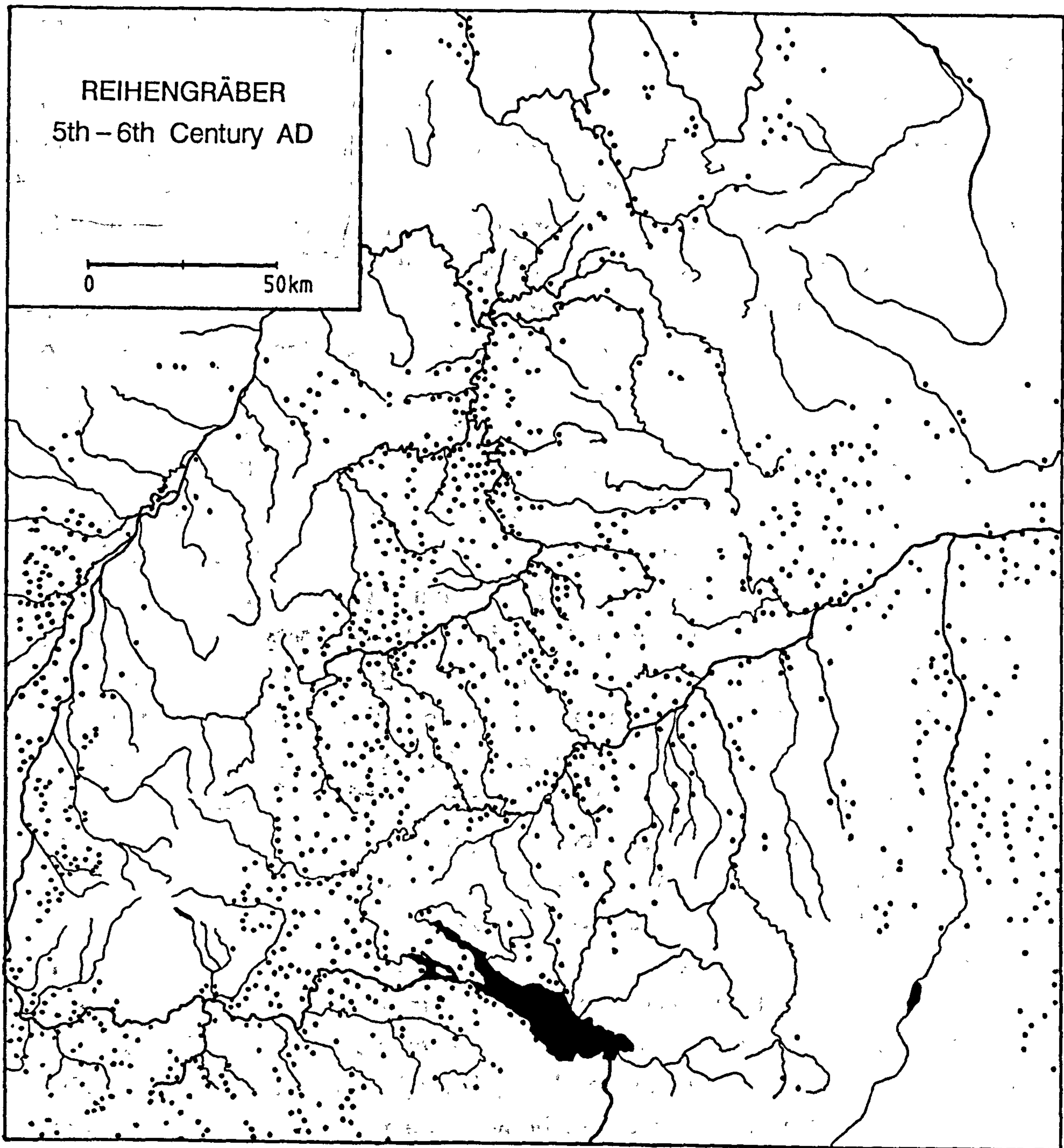


Fig. 5.9 Distribution of Reihengräber in Alamannia (after Steuer).

or to see it decrease from the battles fought against the Romans and subsequent emigration into Alsace and Switzerland or even from the various plagues that numerous writers (e.g. Doehaerd 1971) try to summon up to wipe out vast numbers of Europeans in late Antiquity to make its subsequent history fit their models of economic and social development. But the truth of the matter is that we have no evidence whatsoever to argue one way or the other. A comparison of the situation just before and after the period 260-500 suggests that the numbers were very broadly the same.

A comparison of the occupation of the area in the second and third century with the sixth and seventh century is possible only by comparing the distribution of villas and *Reihengräber* (figs. 5.8, 5.9). We get a visual impression of a broadly similar pattern. Fingerlin notes that the main difference is that in the sixth and seventh centuries moist valley bottoms and the higher regions were avoided. He concluded that the cause was the disappearance of a market economy. On this point I am compelled to agree. The reduction of occupied areas is not at all incompatible with the maintenance or even an increase in population levels, for Roman efficiency meant a more 'economic' use of the countryside. The disappearance of markets necessitated a return to a more self-sufficient form of production. Fewer *patroni* with less extensive coercive powers to exploit dependent peasants on a large scale meant a decrease in specialisation and more labour-intensive agricultural methods were needed. Such increased labour intensity and the lack of specialisation in favour of greater self-sufficiency brought disadvantages. The most obvious disadvantages were that trade outwith the region was less possible and the requirements of labour-intensive farming meant that many of the trappings of civilisation in the form of specialised craft production disappeared. These problems do not mean that Alamannia was necessarily less populated in the fourth and fifth century than it had been in the second and third, despite the abandonment of more marginal land. The Alamannic population may have been no fewer in number than a previous Romano-Germanic population and eaten no less. However, the lower efficiency by which that agricultural produce was created meant that people had to live in timber halls rather than hypocaust-heated stone-built homes, and do with fewer imported luxuries that surplus produce was once traded for.

Coinciding neatly with the appearance of a new burial rite was the conquest of the Alamanni by the Merovingians. In 506/7 the Merovingians and Ostrogoths divided up the Alamannic territories between them. In 536 the Merovingians added that territory previously under the Ostrogothic hegemony. In a letter to Justinian, ca. 545, Theudebert claimed dominion to the Danube and border of Pannonia. In 531 Theuderic defeated and had killed the Thuringian king, Hermanfrith, who had been important enough to have been given Theodoric the Great's niece in marriage. In 555 Lothar I wasted Thuringia because they had aided the Saxons in an attack on the Franks. A border along the Lippe to the Unstrut is considered to have separated Saxons from Thuringians. In the 560s the Franks entered Thuringia to fight the Avars on the Elbe, and in 595 fought the Warni east of the Saale. Alamannic dukes appear to have fought on behalf of the Franks in Italy, and they composed one of Dagobert's armies when he marched

against Samo in Bohemia. Dagobert also appointed Radulf to be duke of the Thuringians. This same Radulf became more or less independent following Dagobert's death and defeated young Sigibert III's army in 639. From Dagobert's death until the beginning of the eighth century, the east seems to have been lost to the Merovingians. In ca. 700, for example, the duke of the Alamanni, Gottfried, dated a charter donating Canstatt to St. Gallen to the year of his own reign, not to that of the Merovingian king. Campaigns had to be fought against the Alamanni in 709 and 712, but it was not until 744 that their opposition was finally broken. Charles Martel undertook no less than seven military campaigns against the Saxons, two against the Bavarians, and parallel to this, actively supported missionary activity in Hessen and Thuringia.

To what extent were the areas of Thuringia, Hessen, and Alamannia affected by their close connection with the Franks before their reincorporation into the Frankish empire by the early Carolingians? Werner (1965) suggested that it stabilised the region, that there were then no more external threats, that the need for defensive sites was gone, and that Merovingian lords practiced their lordship without *Burgen*. Konrad Weidemann (1975c) takes up Werner's thesis but makes the Frankish conquest the cause of many archaeologically detectable changes in south-western Germany at the beginning of the sixth century. Thus he (1975c) notes the introduction of Christianity, the appearance of 'Frankish' material, new mortuary practices, the end of traditional Alamannic pottery, and the break in hillfort occupation. Now the evidence for Christianity in Alamannia in the sixth century north of the Rhine rests on little more than some gold crosses placed on the chest of a few of the deceased, a custom borrowed from northern Italy with which Alamannia had so many contacts. The subject of Christianity in Alamannia before 700 is one about which very little information exists (see Paulsen 1956; Fehring 1979; Christlein 1978). Excepting Konstanz, there are no known monasteries or bishoprics north of the Danube and east of the Rhine in the sixth century and only three remotely possible candidates for the end of the seventh century. As for the appearance of *Reihengräber* in south-west Germany, these had little to do with Frankish conquest and for several reasons. Firstly, *Reihengräber* only appeared at the end of the fifth century in Frankish regions, and there exists evidence to suggest that in Baden-Württemberg there was a comparably early appearance. In fact, Weidemann (1975b, 95) himself claims elsewhere that the appearance of and changes in the *Reihengräber* culture occurred so quickly over the whole territory north of the Alps that the spread cannot be chartered archaeologically. Even to this day the phenomenon of the origin and spread of the *Reihengräber*, appearing in Visigothic France and Spain, along the Danube, in

Bohemia and Lombardy, as well as the Frankish-controlled areas, remains poorly understood. In any case, their appearance in south-west Germany shortly before the sixth century pre-dates the Frankish conquest and can be accepted as having therefore nothing to do with it. The appearance of 'Frankish' objects in the archaeological record is almost inseparable from the appearance of *Reihengräber*, for they themselves are the cause of the flood of this new archaeological information. The fourth and fifth century are very dark, but much of what does appear dateable to these centuries is 'late provincial Roman' material that came from west of the Rhine. Imported material of the following two centuries likewise came from west of the Rhine. But now archaeologists assign it the epithet 'Frankish'. However, that which can definitely be assumed to have been produced in Merovingian Gaul, such as glass or the Vorgebirge pottery, is the unbroken successor of late provincial Roman workshops. The only important change is that of terminology. Although it is not hard to accept that Frankish-produced material should appear more frequently following Clovis' conquest of the Alamanni, it could also be maintained that the *ultra Rhenum* territory maintained stronger connections with their southern Ostrogothic and later Lombardic neighbours. Werner's distribution map of Merovingian coins compiled in 1954 showed not a single coin minted east of the Rhine. Here only Ostrogothic, Byzantine, Lombardic coins, or their imitations could be found. Since then some few Merovingian coins have been found, but if anything, the pattern observed by Werner has now been intensified (Menke 1987).

Only the end of traditional Alamannic pottery production could be accepted as broadly coinciding with Frankish conquest, but otherwise Weidemann's theory of dramatic changes following the incorporation of Alamannic territory into the Merovingian empire cannot be sustained. Thus most of the circumstantial support for the idea that *Hohensiedlungen* ceased as part of the upheaval disappears. If abandonment be suspected, the belief must be grounded in the scarce evidence we do have at our disposal for occupation or lack of it.

It could be said that Runder Berg alone truly offers us sufficiently good chronological evidence to date a break in occupation. Fehring argued in 1972 that Werner's thesis was wrong because he supposed a long continuity of finds from the Runder Berg. However, it is now accepted that occupation ended at the beginning of the sixth century and did not recommence until the latter half of the seventh. There is some localised evidence of burning on the site and of the fifth-century finds. In particular those finds dating to the late fifth century have suffered from exposure to fire. Arrowheads and fragments of iron, interpreted as being broken slivers from the edges of swords, have been found mainly in the

area along the line of the palisade enclosure. This appears to be the debris of the attack and the interpretation of a violent end to Runder Berg has been put forward. To strengthen this view further, a number of hoards of metal tools and ornaments has been found around the site, deposited most probably in the fifth or early sixth century; these are thus seen as safety measures taken before an imminent attack.

With such evidence it is easy to see why Weidemann should leave aside Werner's explanation that abandonment was caused by the lack of external threat and adopt one which saw abandonment as the result of Merovingian military conquest and the expulsion of native princes. Yet to accept that Runder Berg's earliest occupation ended as the result of destruction by a Merovingian army is very rash. Clovis' battle of Zülpich put the clash equidistant between Runder Berg and Paris, and Zülpich was a late Roman *castellum*, not a small enclosure on the top of a hill set in a remote corner of the Erms valley. It seems clear that if the burning and abandonment of Runder Berg had anything to do with the extension of Merovingian hegemony over Alamannia, it would have been on a local level and not the consequences of a passing royal army. But if the burning of Runder Berg was the result of a local conflict it need have had nothing to do with Merovingian overlordship. One can scarcely envision peaceful Alamannic chieftains and princelings who never fought each other, but only with 'external' invaders.

Of course, Werner and more lately Weidemann have based their arguments on more than just Runder Berg. The series of finds at Glauberg also ends with the turn of the sixth century. At Glauberg this is perhaps meaningful, for the finds for the fourth and fifth century are numerous, while finds for the early sixth century are very few and the following hundred and fifty years are completely unrepresented.

We have seen how frequent it is that a hillfort might yield only a few belt-buckles, brooches, or several sherds of pottery of late Roman date. Three or four finds from the fourth and fifth century and none from the Merovingian period are not even remote evidence for a break in occupation. If it were, however, the discovery of a pair of Merovingian finds should, conversely, be proof of continuity. If it were, then continuity might be recognised at Goldberg, Hesselberg, Staffelberg, Aschaffenburg, Reisenburg, and Quedlinburg. There are also a number of sites that yield a few stray Merovingian finds as their earliest medieval material, as we have seen. For nearly as many hillforts as were abandoned, we could argue new ones were being founded.

Underlying Weidemann's explanation of hillfort abandonment is the

implication that because a Merovingian king successfully conquered parts of Germania they became replicas of the Frankish kingdom, at least in the structures of power. Frankish nobles who replaced native princes are supposed to have lived on Frankish-style villas. Where else did they live if not in hillforts? In fact there is very good reason for believing that Merovingian hegemony made no impact on the social and political conditions of Germania at all.

Firstly, there was no coinage in Germania. No coins were minted there until well into the Carolingian period. There remains a great deal of uncertainty over the role of Merovingian coinage: did it indeed function as money, or did it have a more specialised role in social transactions, such as payment only of wergeld or taxes? Did it function as part of the *grande commerce* such as Pirenne envisioned it? Even if one should choose to be as contemptuous of Merovingian mercantile trade as Latouche (1961) or ignore it as does Hodges (1982), one must admit it certainly had some importance in Gaul, whereas it was non-existent in Germania.

Just how urban Merovingian cities were is also a source of debate. In Germania, however, adjectives such as '*stadtähnlich*' can only be used to apply to the size of some enclosures, for the necessary prerequisites for urban development were missing. Without currency, commerce in the sense of that undertaken by entrepreneurial traders could not have existed. Without bishoprics, monasteries, or royal courts there was not even the more common cause for urban concentrations in the early Middle Ages, namely bureaucracy and large-scale exploitation of the rural countryside. Pope Zacharias wrote to Boniface warning him that new bishoprics should not be set in *vici* or small cities in Thuringia. Boniface's description *olim urbs paganorum* was undoubtedly designed to pre-empt any such papal disapproval when he wrote asking for his new bishoprics to be confirmed in Erfurt, Würzburg, and Büraburg. But Boniface had little choice in the matter; there were no former Roman cities to choose from.

The settlement geography of Germania was different from that in Gaul. The social organisation was also different. The distinction was perhaps seen by Gregory of Tours and Fredegar, for both speak about the people across the Rhine as being distinct, and above all barbarous. The people across the Rhine were not Christian, but the 'wildness' of these peoples must have had deeper causes. Just why monasteries should be lacking in Alamannia and Thuringia in the sixth and seventh centuries perhaps requires deeper consideration than it usually receives. For the sixth century, the argument that the eastern territories were all pagan and clearly needed to be Christianised before monasteries could be established is perhaps acceptable. Not so for the seventh. Archaeological evidence has already shown the existence of several wooden churches in Alamannia during the

seventh century (Fehring 1979). The Irish missionaries following Columbanus preached in the heathen territories across the Rhine in the seventh century and were perhaps partially successful. The correspondence between Boniface and popes Gregory II and Zacharias shows that the inhabitants of Thuringia and Bavaria were considered more often to be bad Christians or apostates than actual pagans. But if the Irish mission failed in the way the Bonifacian mission did not, its failure was almost certainly not, as Talbot (1954, ix) claims, because Irish discipline was too severe, because the zeal was marked with intransigence, because there was a lack of interest in creating a stable hierarchical ecclesiastical organisation. More plausibly it could be argued that following Dagobert's death, the lack of a strong supportive and imperialistic Merovingian monarchy meant that Irish missionary activity in Thuringia or Alamannia had no choice but to rely on burning enthusiasm to win over the support of local nobles. It was not that the Irish discipline was too severe but that it lacked the backing of a severe military force. The followers of Boniface were more fortunate in secular backing. Their close contacts with the aggressive military Frankish royal houses may explain better the success of the English missions.

There may just have been one other reason for the lack of monastic foundations before the eighth century in Germania. And this one I believe to be more important. Merovingian kings and nobles may not have had estates east of the Rhine with which to endow religious houses. Charles Martel founded Reichenau monastery on lake Konstanz as, according to many scholars, part of a political move against the recently conquered Alamannic duke. It is interesting that the Merovingians never tried such a manoeuvre, although a series of monasteries within Gaul was closely connected with the royal family, St. Denis being the final and best known. It is just conceivable that appropriate estates were not available east of the Rhine because such centralised agricultural villas may not have existed there. Of course, to suggest that the Thuringians knew only scattered and autonomous farmsteads, innocent of any lordly exploitation, must be going too far. However, in this respect the east German territories must definitely have been more 'backward' than Gaul.

The main reason for the lack of evidence for royal estates east of the Rhine is the lack of charters, which in turn is the result of the lack of monasteries to which they would have been given. Thus the argument certainly has the danger of circularity: no monasteries, no charters proving royal estates; no estates, no monasteries. However, there is more positive evidence. With perhaps two exceptions, the Merovingians are never recorded as having crossed the Rhine except to wage war. An early Frankish king was killed while 'taking a walk in

Buchonia wood' (near Fulda), and Dagobert is recorded in the *Vita Arnulfi* of having accompanied the saintly man when he crossed the Rhine, although the reason was not given. Furthermore no charters were ever confirmed across the Rhine and there is no mention of kings on campaigns as having stayed anywhere other than in armed camps. It could be noted that in Charlemagne's travels, about which we are particularly well informed, he seldom crossed the Rhine and then almost exclusively to fight battles and almost all the palaces in which he stayed are recorded as having been built by him.

It is clear that there is only negative evidence to say that Merovingians had no estates *ultra Rhenum*. Surprisingly Schlesinger (1975) is almost alone in having noticed this absence. The reason must be that the belief in the existence of royal villas and military sites and the royal Merovingian creation of new settlements is firmly entrenched. This idea has been found in place-name studies and as early as 1875 in the work of W. Arnold. It is difficult enough to trust the dating of place-names, but to use place-names, especially such common names as *-heim*, *-ingen*, *-hausen*, or *-dorf* to write political history is surely quite unwarranted. Schlesinger (1975, 9) notes that between 213 AD and the Carolingian period there exists only one single documentary reference to the position of Hessen *vis-à-vis* Merovingian Gaul. Furthermore, Hessen is considerably poorer in burial evidence than Baden-Württemberg, and has fewer excavated farmsteads than either southern or northern Germany. We have seen how poor the archaeological evidence for *Burgen* in the period is and yet Nitz (1963 and 1983) believes he can see the remains of the foundations of settlements of rent-paying peasants planted by Merovingian kings on the basis of place-names and their relationship to the main routes through Hessen! The whole system was, apparently, protected by fortifications distributed at regular intervals and garrisoned by royal troops.

Besides the fact that there is no basis for such a theory beyond an active imagination, there is also the problem of how we conceive of these foundations actually working. Just what form did the supposed payments (*Zinsen*) of these settlers take? It is clear that the Merovingian kings did not plan to visit them, where they could then consume the agricultural produce. We have seen that the Merovingian kings had their densest concentration of estates exactly where they spent most of their time, and vice versa. Fewer estates were held in Provence or Aquitaine and these regions were seldom visited. Which, however, was cause and which effect, or was it a vicious circle? Whatever the answer, it was clearly impractical for agricultural produce of distant estates to be transported great distances. Exceptions occurred when the distant estates could provide a specialised product, such as wine. East German estates could not have been

expected to produce any specialties that were not more readily available in Gaul and the transportation of vegetable produce was clearly unrealistic. Further, before the Carolingian period the lands beyond the Rhine were non-monetary; estates there could not have *paid* their rent. What remains then is the possibility of renders of livestock. Fredegar (c. 74) tells how Dagobert cancelled the annual tribute of 500 cows from the Saxons which Lothar I had first demanded, perhaps in 556. We might imagine that Lothar managed to exploit Thuringia in a more refined manner than the 500 cow tribute demanded of the Saxons who remained beyond the Merovingian hegemony. But not much more. Certainly there was no network of estates in the style of the *Capitulare villaris*. Lothar could probably not have required eggs, chickens, fish, vegetables, or even grain and sheep from Thuringia, only cattle, horses, and on occasion perhaps gold and silver in weight.

The archaeological record may provide more evidence that such an estate-based system of exploitation did not exist. We might compare the residential, agricultural, and artisanal buildings within *Burgen* with those of excavated farmsteads and hamlets to see if the differences suggest the presence of elites. We might start by considering some of the characteristics of elite dwellings and settlements that we should expect given our suppositions about early medieval lordship. Firstly, a common characteristic among social elites is to dwell in residences which distinguish themselves from 'common' architecture either in size, care in construction, materials employed, or in spatial setting, or all four. In most cases stone was seen as more impressive and thus more appropriate to rulers than wood, but exactly the reverse was true in Iceland and Greenland during the Viking period. There, because of the scarcity of wood, and the poorer quality of the local stone, wood was occasionally reserved, for example, for the west facade of churches to make them more imposing, while the rest was constructed of stone or stone and turf. Secondly, wherever we can detect evidence of the use of very large amounts of man power, we can safely exclude the possibility of some primitive co-operative and assume that the leadership and organisation was 'provided' by a ruler of some description. Thirdly, we can assume that imitation of social superiors was practised. If, as seems permissible, we assume that Roman civilisation was seen as being superior, then those sites which seem to ape Roman ways most are probably those of the highest ranks of German society. In this sphere we can look for imitation of Roman building styles and techniques and life style in the form of dress, eating habits, and use of a multitude of Roman material goods. Fourthly, we should expect some form of control over commodities; the so-called redistributive role of chieftains. This might take on the form of extremely large storage capacity, either for grain in

granaries or herds in byres to mention only the most common. Or it might take on the form of control of access to precious metals, including mines, trade and production. Rather than the quasi-capitalist merchant trader of the Roman Empire or Middle Ages, we should expect something closer to the activities of the Vikings. In the twelfth and thirteenth centuries many trading ventures were financed, organised and often led by the *goðar*, the chieftains. Even when traders of a more entrepreneurial nature appear, they frequently appear to have had no rights to sell or trade without dealing directly with the *goði* first and then trading under his supervision. Thus even when the initiative originated from outside the chieftains' hegemony, they maintained control of access to prestigious and even vital goods, thus helping to maintain their social and political ascendancy (Samson forth. b). For the fourth and fifth centuries we may assume that Ammianus' *reguli*, among other documented and postulated leaders, attempted to monopolise access to Roman goods. Fifthly, we can assume that there were many people living nearby, including servants and perhaps retainers and dependants.

As a test we could apply these criteria to the best-excavated Ottonian palace, Tilleda, and find that they fit perfectly (fig. 5.10). It was quasi-urban with nearly two hundred *Grubenhäuser*. It had large grain-storage capacity, thought to hold grain from dependent estates, particularly Dullide. It revealed a wide variety of manufacturing processes of different materials, including ivory, horn, bone, iron, copper, bronze, lead, textiles, and pottery. It yielded evidence of large-scale work that would have demanded huge amounts of labour in the form of earth moving for the construction of ramparts. And finally it revealed marked distinctions between the stone-built residential quarters presumably of the king and the timber halls and *Grubenhäuser* elsewhere. The 'royal apartments' were, furthermore, in a separate enclosure at the highest point of the site.

We know that Tilleda was the residence of one of the most powerful men in tenth-century Europe. It is not surprising then if all the criteria hold good. Not all, but some of the criteria hold good for the *Burgen* of fourth- to seventh-century Germania.

The comparatively large-scale construction of ramparts demanding large amounts of labour is found at Gelbe Bürg, Urphar, and Glauberg, although not at Runder Berg where the ramparts were much smaller affairs.

We may speculate that there was a large population at the large hillforts although, given the propensity for German archaeologists to interpret them as unoccupied refuges, there is unlikely to be much agreement in speculation. The large hillforts also lack good extensive excavation of the interiors; those that

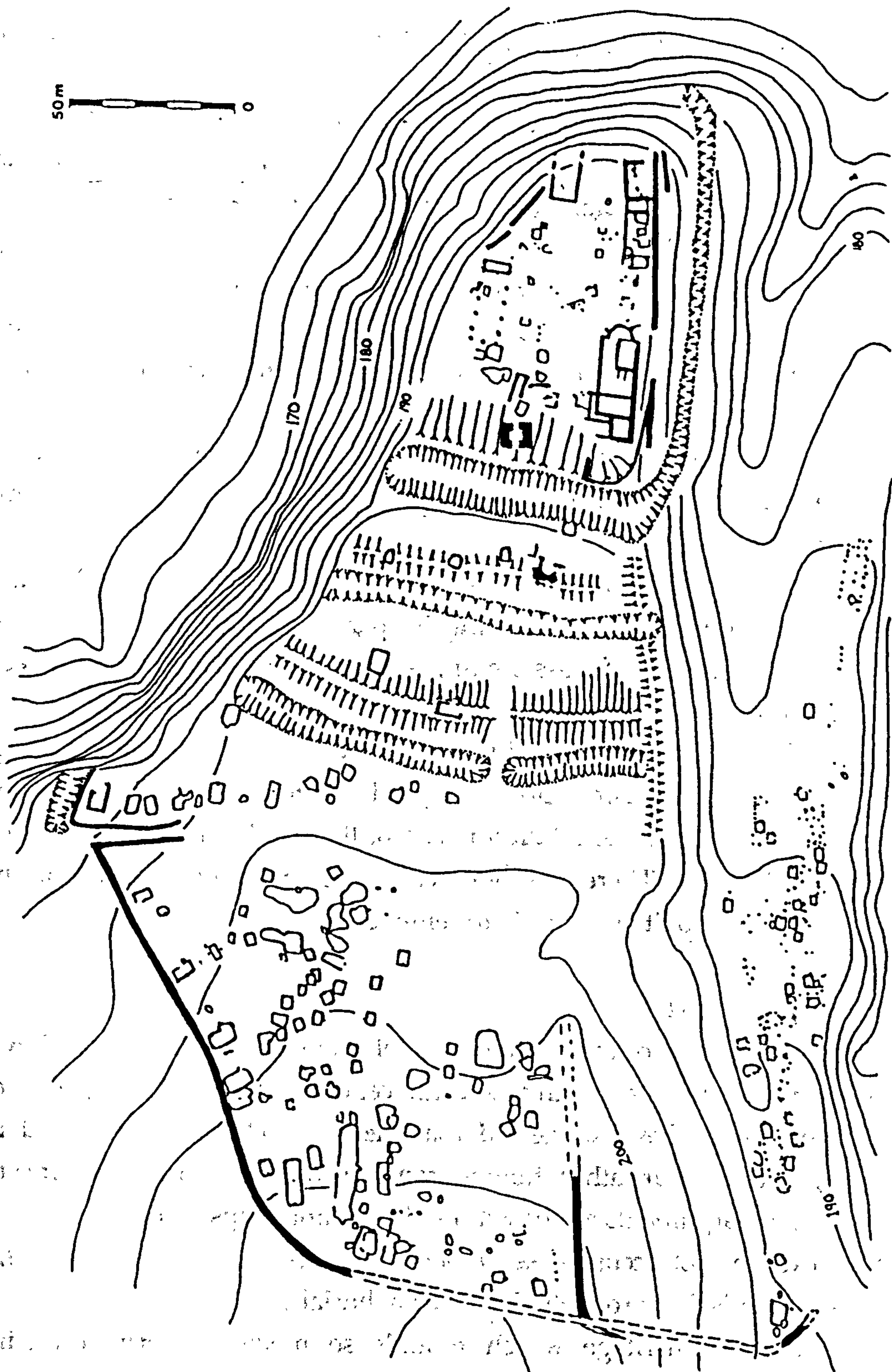


Fig. 5.10 Tilleda, a major Ottonian palace (after Grimm).

figure in this chapter have largely been subject to investigation of their ramparts. Certainly Runder Berg was not densely populated. Given its size it could not have been populous even with very dense occupation. Nevertheless it still had accommodation equal to or greater than most of the excavated contemporary farmsteads.

Evidence for distinctive noble architecture is likewise thin. Only at Runder Berg is there enough evidence to discuss. There we may note buildings in stone and the use of some mortar, which are so far unique for pre-Carolingian Germania. There we find the use of stone and mortar limited to churches and high status burial chambers, and then usually in churches. At Runder Berg we also find that considerable amounts of labour were expended in cutting foundation post-holes into the rock for timber buildings, and one such timber building is among the most impressive known from this period. Finally there is the hierarchical use of space: a timber building on the summit, stone buildings around the stone enclosure wall, and the different activities beyond the rampart and on different terraces.

The evidence of artisanal production is met both at Runder Berg and Glauberg. Most impressive, and setting it apart from most sites, Runder Berg yielded evidence not only of glass melting – associated in Britain with the seats of barbarian warlords – but even of glass blowing. Not only was there bronze casting, but the belt-buckles were of the chip-carved variety so popular along the Roman *limes*, and often misinterpreted as produced to outfit the Roman army. In fact the amount of Roman emulation is one of the striking things about Runder Berg (Fehring 1987, 96). There was, however, less evidence of long-distance trade than we might wish to fit our criteria for elite residence.

Concluding Remarks

There is some evidence of the residences of elites in Hessen, Thuringia, Bavaria, and Alamannia during the fourth to seventh centuries. The evidence, however, is poor. So poor that I have suggested that there is nothing to be gained from adding to gazetteers, but rather there is from weeding out most of the sites from those lists. Many arguments are based on distribution maps that probably do not reflect the existence of occupied settlements enclosed by massive ramparts, but rather of stray finds that probably derive from burials.

The intellectual heritage which moulds so much of German medieval historians' work equates history with political history, and changes in social organisation with the consequences of political history: royal directives, usurpations, wars, conquest, and 'constitutional developments'. Some scholars

like Dannenbauer and Schlesinger have continually tried to push later medieval political and social structures back into prehistory. All this means that far too many articles have been produced dealing with supposed defensive systems of fortifications, guarding frontiers and roads, the conscious result of political planning and the threat of foreign invasions. It means that sites like the Runder Berg become precocious castles, held by forerunners of medieval castellans, the outposts of Frankish military lieutenants.

The existence of an aristocracy, of a society dominated by warrior elites, cannot be denied. I have only fleetingly touched on the nature of their authority and power, suggesting that it was less firmly based on land than in the Middle Ages and relied more on personal ties of dependency. I believe this form of authority was less exploitative and geographically more immediate. Given the poor quality of the evidence, this cannot be stated with certainty. But it does go some way to explaining the major distinction that can be dimly discerned between the residence of elites in Merovingian Gaul and Germania: the latter tend to be more martial, but less sophisticated in building materials and size, and are much less clearly differentiated from the majority of farms. But that the disparity between their forms of authority, power, and wealth and those of Merovingian nobles in Gaul was narrowing is evidenced by the rapidity with which the Carolingian villa, or *curtis*, became established in Germania during the ninth century, as we shall see in the following chapter.

CAROLINGIA

Chapter Six

Royal Carolingian Palaces: a Gazetteer of Some Important Sites

The Carolingian period marks a great increase in the amount of evidence available for the study of early medieval villas and estates. First and foremost the charter collections and polyptychs of great monastic houses allow investigation of estate management and ties of social dependency. More casual information about buildings, such as the building materials used or the existence of enclosure walls, is available in the written sources than survived from the Merovingian period. And yet these written sources need not be so intensively mined, for the wealth of archaeological data is much increased. In this chapter I present only the best preserved evidence pertaining to the buildings of royal Carolingian villas.

The royal bias is to be explained several ways. Study has overwhelmingly concentrated on royal palaces, as was discussed in chapter one. Royal buildings were presumably the best built in the period and are perhaps therefore the best preserved. But there is one last cause, which will be discussed in the following chapter: contemporary non-royal villas have been wrongly interpreted or classified as royal. A favourite explanation of these other sites is as a royal stronghold or fort. This appears widely in the secondary literature, the result of an inherent overemphasis on the monarchy as a mainspring of historical change. Less impressive remains are designated 'Carolingian farms' by archaeologists and never seem to break out of the confines of excavation reports into historical syntheses; they languish unconsidered by historians.

Although no more than a beginning can be made in the next chapter to redress the balance, it seems wise to begin by looking at the comparatively good evidence for where the dominant men in eighth- and ninth-century Europe lived. To that end, this chapter consists exclusively of a gazetteer of less than a dozen sites and one text. It naturally can do little more than collate, translate, and summarise other scholars' work. This is no doubt valuable in itself, for the only other recent attempt at something similar, found in the report on the excavation of a royal

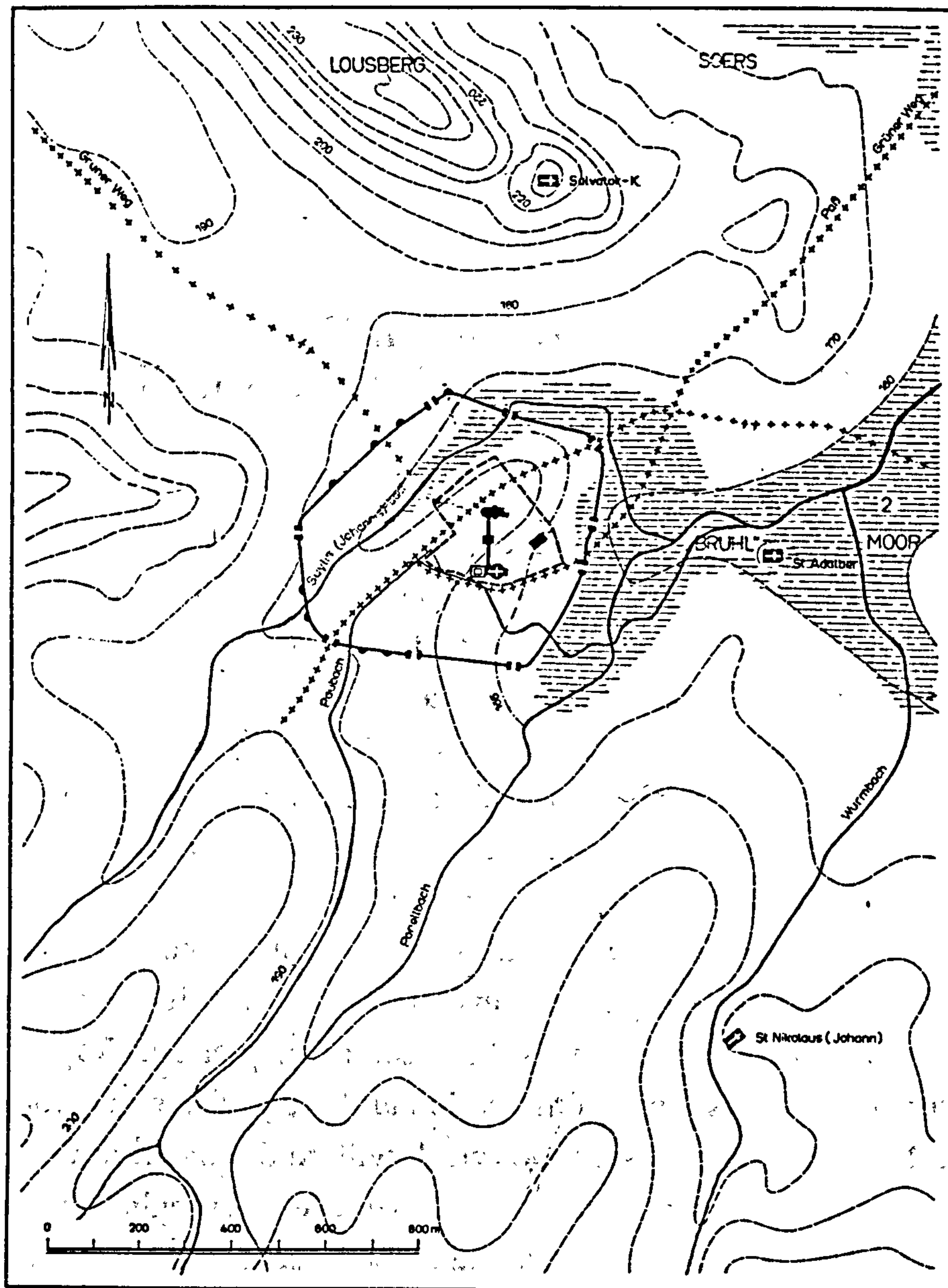


Fig. 6.1 The topographic setting of Aachen palace (after Gauert).

palace in Northampton, is marred by several errors (Williams 1985). In addition, however, I have not only brought the entries up-to-date (important for Ingelheim and Paderborn), but have also introduced some of my own interpretations, including two reconstruction drawings. Hopefully these interpretations have a lasting value for being minimalistic and for being drawn from comparative knowledge of other, contemporary palaces and their precursors.

Aachen (Nordrhein-Westfalen). At the northern foot of the Eifel, Aachen lies within a small semicircular valley about one and a half kilometres in diameter

(fig. 6.1). A series of spurs descend from hills to the west and south and lie some forty metres above Aachen's centre. A long hill nearly 100 metres above Aachen lies to the north, thus concentrating the focus of the valley to the east. The palace site lies on a slight eminence which projects to the east, thus ensuring that it lies on well-drained soil and obtains a good view.

Aachen did not lie on a major Roman road. A road from Jülich to Luttich, running east-west passed through Aachen and a small route came from Heerlen in the north and passed through Aachen, presumably headed into the Eifel. On the western side a road connected the site with a route to Maastricht. Attempts have been made to reproduce a Roman grid plan within the city on the grounds of the street plan, but this seems to stretch credulity. A Roman presence is certainly documented by the excavation of Roman baths with legionary tiles dating its origin to 89 AD. Its destruction has been dated to 375 AD.

Continuity through the Merovingian period has not been documented historically, but although no Merovingian finds are recorded from excavation, so too are Carolingian finds almost non-existent. Excavation within the Minster has revealed evidence of three altars predating the present one. The earliest is on the same alignment as earlier Roman buildings, the second and third follow a different alignment, which the Minster roughly follows. The pre-Minster churches were found lying over a 1.2 metre thick layer of Roman rubble. The form of the church contemporary with the second altar has been reconstructed as circular and only 8 m. in diameter, or perhaps it was an apse. Thordemann (1965) believes the predecessors could have been Merovingian, for Angilbert wrote at the start of the ninth century that relics were gathered here by earlier kings (*ab anterioribus regibus*), which almost certainly was a circumlocution for Merovingian kings, about whom the Carolingians were still sensitive. The minster is often erroneously called the chapel, for from its inception the church was intended to be a cathedral and Charlemagne probably had a chapel proper elsewhere within the palace which will be discussed later.

The RFA record the celebration of Christmas by Pepin at the *villa* of Aachen in 765. Although it often appears in the literature that Pepin reused Roman buildings, there is absolutely no evidence to support this claim. Indeed, the altar immediately predating that of the Minster which Charlemagne had built was almost undoubtedly where that Christmas mas was celebrated in 765. Presumably it was levelled because of Charlemagne's intention of raising Aachen's status to that of a bishopric and an appropriately

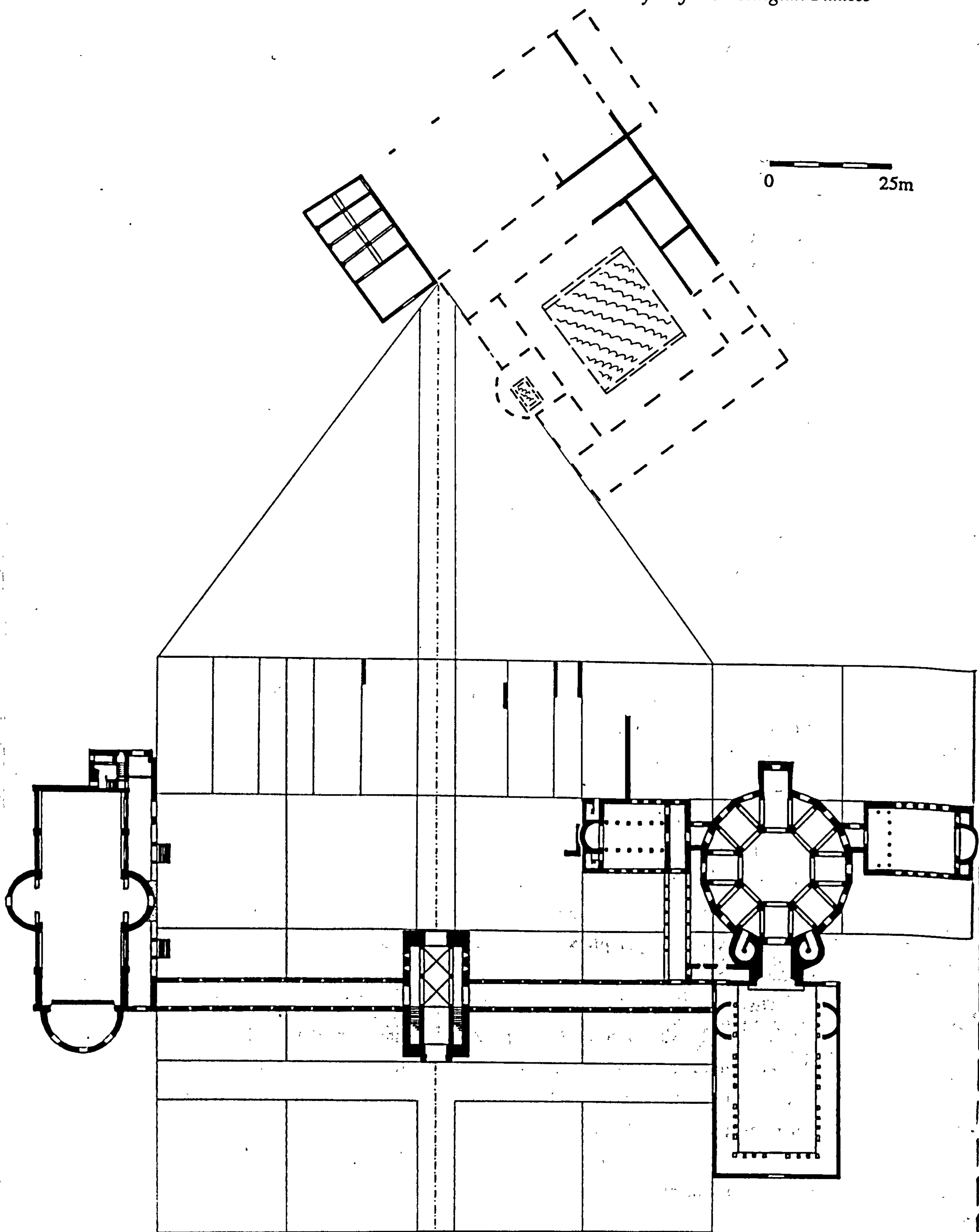


Fig. 6.2 The Aachen palace complex with modular grid (after Hugot).

monumental cathedral was necessary to adorn the palace complex. Further information about the villa as it existed before Charlemagne is unfortunately not forthcoming, and what may be archaeologically reconstructed of the palace complex (fig. 6.2) is usually attributed to the reign of Charlemagne. There is, however, a general dearth of datable archaeological evidence and reliance on mortar matrices as chronological indicators is not satisfactory. We cannot date or phase the site accurately. It is sufficient to accredit them to Charlemagne, his son, grandson, or even his father.

It is generally accepted that Aachen was not fortified until it received its wall from Frederick Barbarossa in the twelfth century. A section of rampart 9.6 metres wide, built of greywacke stone with mortar containing ground brick was found in pre-war investigations and, as we will see, such characteristics correspond with what is accepted as typical of the Carolingian buildings. The discovery of a denarius of Louis the Pious within the rampart wall provides a *terminus post quem*, but because the discovered wall lay on the rampart circuit of the Barbarossa wall, it has generally been accepted as being part of it. Otherwise, in the immediate vicinity of the palace complex, small stretches of walling and ditches have been postulated by Hugot as delineating the complex without being defensive, although these features were discovered in such restricted areas as to make them almost meaningless. Furthermore, no postulated enclosed area corresponds even remotely to the known cathedral 'immunity', as the two do at Paderborn (fig. 6.9), further weakening the credibility of the existence of such an enclosure.

Turning to the palace complex itself, Hugot has recognised a lay-out that was clearly planned, using the Carolingian foot (one third of a metre) as the standard measurement. Figure 6.2 shows that the hypothetical modules fit too precisely to be accidental. This allows the speculative reconstruction of a continuous barrier in the form of facades of contiguous buildings (or perhaps just a simple wall) parallel to the corridor-like building connecting the royal hall and church, thereby creating a secluded courtyard between the two monumental buildings. Hugot describes the major road which runs west-east through the complex as the *via principalis* and thereby perhaps implies an indebtedness to Roman parallels, particularly Roman forts, which it does not deserve.

The Great Hall. The hall (fig. 6.3), which now exists as the foundations of the predominantly Gothic *Rathaus*, was in plan a large rectangular hall with three apses, forming a construction known to late Antiquity as a *trichorium*. There is no evidence that the interior was divided into separate rooms during

the Carolingian period, or that the building was aisled. The foundations and walls were built of greywacke stone. In the foundations a brown mortar was used, in the walls a reddish mortar, its colour obtained from the admixture of ground brick or tile, a Roman technique. The measurements were:

| | actual measurement | Roman feet | ideal measurement |
|----------------------------|-----------------------|---------------|----------------------|
| length: | 47.42 metres | 160 | 47.36 metres |
| exterior width: | 20.76 | 70 | 20.76 |
| interior width: | 17.2 | 58 | 17.17 |
| wall width: | 1.78 | 6 | 1.78 |
| foundation width: | 2.4 | 8 | 2.36 |
| western apse ext. radius: | 8.9 | 30 | 8.88 |
| northern apse int. radius: | 7.67 | 22.5 | 7.66 |

The amount of deviation from the ideal measurement in terms of Roman feet, it will be noticed, seldom exceeds .1%, according to Hugot. However, it is meaningless to talk of 'deviation' for a supposedly exact measurement of a Roman foot still varies by at least a per cent in the writings of different modern scholars, and greater precision probably did not exist in Antiquity. Moreover it should be noticed that all the actual measurements are consistently longer than Hugot's 'ideal' measurement. That is to say, the measuring devices used by the builders were probably as precise as any metre tapes used by archaeologists or builders today, and the actual building to plan even more precise. Hugot can find no reason for the use of the smaller Roman foot rather than the Carolingian foot which governs measurements in the Minster, although one immediately plausible reason is that they were not strictly contemporary constructions. To that end, it may also be noted that the *porticus*, the corridor building connecting the hall and church, is also based on the Roman rather than Carolingian foot. It was surely built after the hall and church. Presumably the architects who planned the Minster were different from those who created the hall and *porticus*, perhaps the latter were built slightly later. This upsets the nice progression of the use of Roman to Carolingian measurements, and is further complicated by the fact that the palace lay-out modules are in Roman feet. Could the layout plan have been devised only after the Minster building had begun?

The exterior, it will be noticed, has been reconstructed to bear a striking similarity to the Basilika in Trier (fig. 5.1). The facade of the Aachen basilican hall has been lost except for a short stretch on the south side where two bases

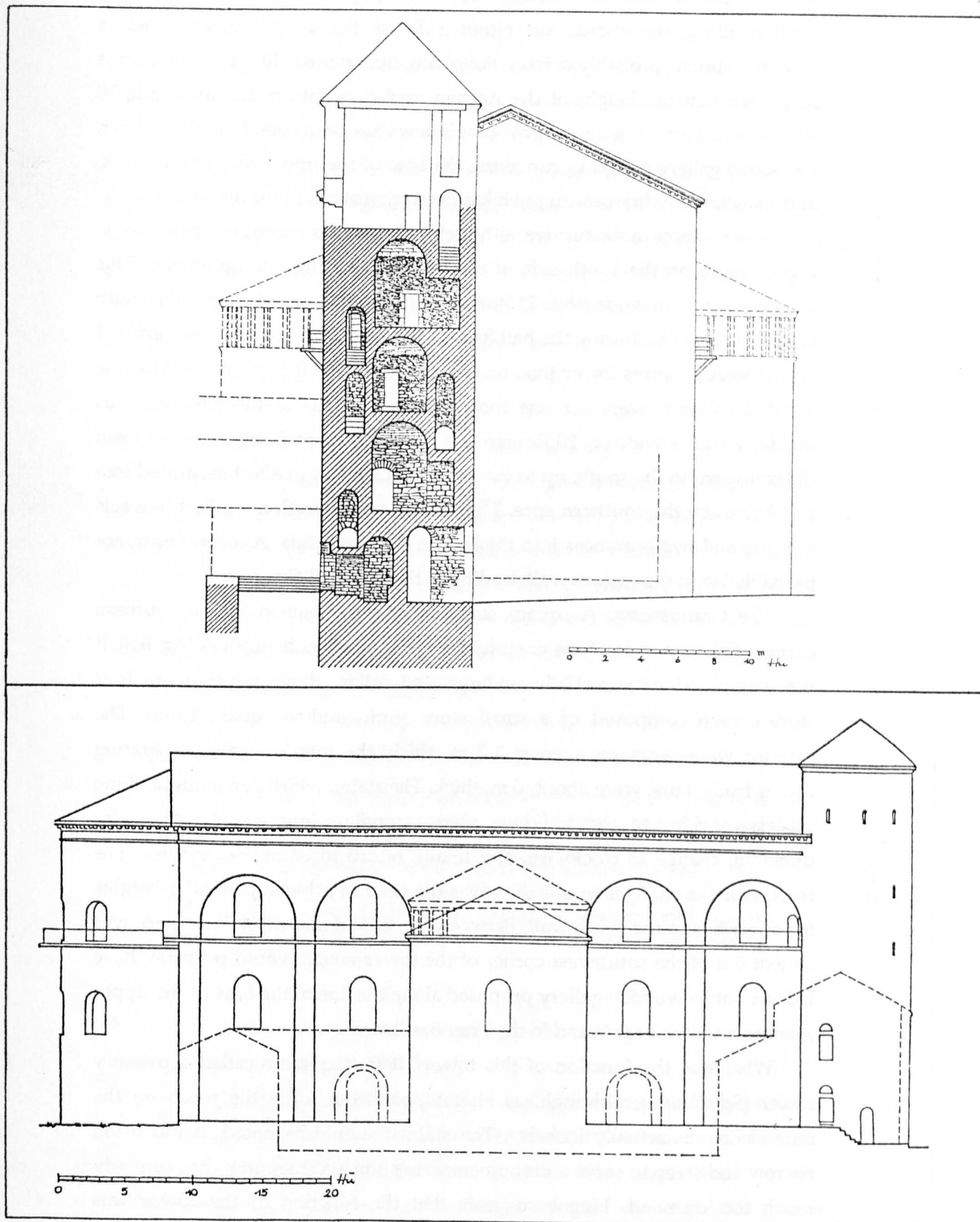


Fig. 6.3 The Granusturm in section and the hypothetical, reconstructed aula regia based on the Trier Basilika (compare fig. 4.1) (after Hugot).

of the pilasters have been discovered 7.05 m. apart and 1.5 m. wide, thus exactly filling the facade on either side of the central apses. Such a reconstruction is probably correct. Following the Trier Basilika as a model, it is suggested that the height of the Aachen basilica would match its width, 70 Roman feet. Further, a double row of windows has been added to mirror Trier, a wooden gallery added to run along the base of the upper row of windows, and a shallow Mediterranean pitch has been reconstructed for the roof.

No entrances now survive although at least two entrances seem likely: one or more on the south side of the building, exiting into an arcade. This arcade was 6.21 m. wide (thus 21 Roman feet). Its wall lay adjacent to the south facade of the hall, offering the hall further support, for on this side the ground was at least 2 metres lower than on the northern side. It is suggested that the arcaded columns were set one foot in from the side of the passage, thus making a usable width of 20 Roman feet. Steps would probably have led from the courtyard to the south, up to the arcade, which was in effect separated into two halves by the southern apse. Thus in the reconstruction we find two sets of stairs and two entrances into the hall on the south side. A further entrance probably lay in the eastern wall, leading to the so-called *Granusturm*.

The *Granusturm*. A square stair tower was attached to the southern corner of the east side of the great hall, most of which still survives (fig. 6.3). It was composed of a partially underground cellar, above which were four stories, each composed of a small stone groin-vaulted square room. The exterior walls were on average 1.3 m. thick; the interior walls, separating rooms from stairs, were about .6 m. thick. The stairs, which run straight along successive sides of the building, start ascending in a counter-clockwise direction, change to clockwise, and finally return to counter-clockwise. The reason for the changes in direction was the need to achieve particular heights for doorways in the south wall. Between the second and third floor there was an exit out of the southwest corner of the tower which would probably have led out onto a wooden gallery proposed along the line of the base of the upper row of windows, as is found in the Trier Basilika.

What was the function of this tower? It is frequently called a treasury tower (*Schatzturm*), although, as Hugot points out, only the room on the second floor is actually lockable. The stair, it should be noted, is much too narrow and steep to serve a monumental function and the rooms are similarly much too cramped. Hugot suggests that the function of the tower was primarily that of access: to the roof, to the gallery, and to the first floor of an adjoining southern range of buildings, which has subsequently disappeared

but the blocked Carolingian doorway can still be easily seen in the stonework.

The Porticus. Connecting the royal hall and the Minster *atrium*, stretched an amazing construction 120 metres (450 Roman ft.) in length and 7.4 m. (25 Roman ft.) in width (fig. 6.4). The ground floor walls were 1.3 m. wide and were pierced – apparently both sides, west and east – at intervals of 4.4 m. (15 Roman ft.) by 10 cm. wide slit windows. The ground floor was barrel vaulted and there is no evidence that the resulting tunnel-like structure was divided into compartments. Access was achieved at either end and from the central gate-hall. Hugot believes that the military character of this building suggests something like a billet for a garrison. The military character can be pushed too far, for as the building approaches the higher ground on which the royal hall is situated, the slit windows are found increasingly close to the old ground surface, until less than a foot above it. Part of the importance of this building is surely to be located on the first floor, where the long passage connected the arcade of the royal hall with a first-floor door into the Minster, which led into the tribune where the throne sat. The passage was lit by a series of small triple windows, in effect forming a two-storied arcade. Einhard mentions this construction in his *Vita Caroli* (4.32) as a portent of Charlemagne's death: 'The immensely strong *porticus* which he had constructed between his palace and the cathedral (*porticus, quam inter basilicam et regiam operosa mole construxerat*) came crashing down to its very foundations one Ascension Day.' The RFA record the occurrence more fully and presumably more accurately *sub anno* 817 (thus making a poor portent of Charlemagne's impending death):

When the emperor left church on Maundy Thursday after the holy office was over, the wooden arcade (*lignea porticus*) through which he was walking collapsed on top of him and knocked him to the ground, with more than twenty of his companions. This happened because the arcade was made of shoddy material. The worn-out and rotten cross-beams (*transtra*) could no longer hold up the weight of the framework (*contignationem*) and wainscoting (*tabulatum*) above them.

Thus one function of the *porticus* was as a private passage for the emperor to come and go to and from the Minster.

Another straightforward explanation of this enormously long arcade is that it serves as a barrier into the palace courtyard. The facade it presented to the outer world is quite forbidding and would have easily kept out the unwelcome (fig. 6.4). Access into the palace complex was regulated by the gate-hall in the centre of the long *porticus* (fig. 6.2). The ground floor of the hall is purely dedicated to access: the axis of the building accommodates a wide

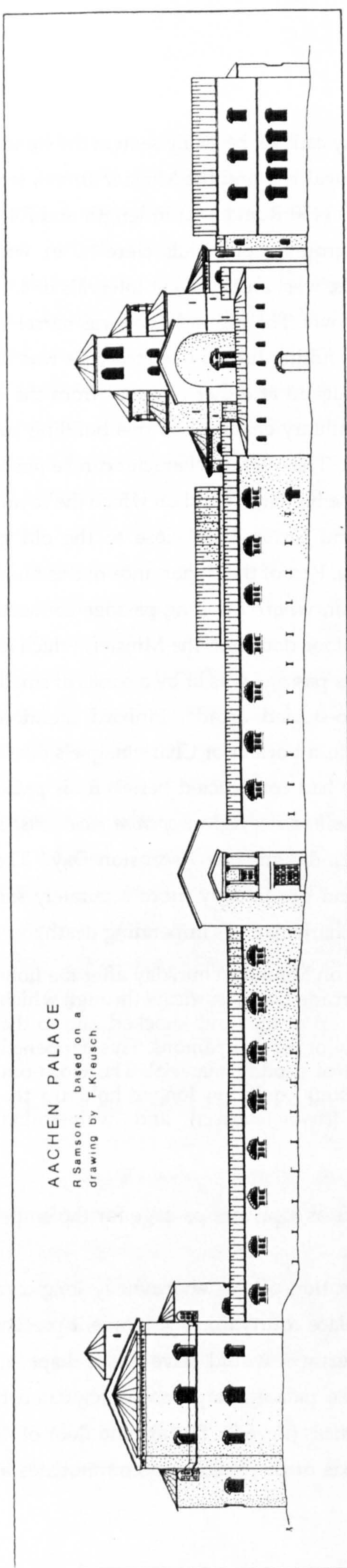


Fig. 6.4 Reconstruction drawing of the Aachen palace complex from outside, facing east (redrawn after Kreusch).

(3.5 metre) passage, which forms part of the main roadway through the palace complex, and entrances to the ground floor of the *porticus*, which seem unnecessarily wide if designed only to accommodate men and would have allowed entrance to small carts so that the ground floor of the *porticus* may have been used for storing goods. Furthermore, there was access from the ground floor of the gate-hall to the first floor in the shape of two staircases.

The Gate-Hall. A further pair of stairs led to the first floor of the gate-hall from outside, in the courtyard. The first floor of the hall remains largely conjectural. What is proposed is a large rectangular hall with doors connecting the upper floor of the *porticus*, an apse at the eastern end of the hall, and a balcony directly over the entrance to the courtyard at the western end. The function of the first-floor room of the gate-hall is sometimes taken to be a judgement hall. The parallel can be drawn to the Chalke palace in Constantinople, where the Byzantine emperor sat in judgement in the entrance hall, an image seemingly drawn from the Old Testament. One immediate advantage of such a position for a judgement hall was that the accused could be brought in without otherwise defiling the palace complex. It was not uncommon for Frankish kings to forbid their subjects accused of serious crimes to come into their presence before being tried and cleared. It further seems probable that a balcony existed above the gateway, from which pronouncements of the judgements of the court could be made and on which the king could make public appearances, and perhaps distribute alms, an activity mentioned by the *Lives of Charlemagne*, and the function ascribed to an upper storey room of a stone-built gate-tower of the royal villa of Asnapius by the *Brevium exempla* (see the end of this chapter).

The Minster. The oft-described church needs only a brief description here, as its architecture has long been the object of study. The main entrance to the church was through an *atrium*, measuring 76 by 84 feet. Arcaded on three sides, the *atrium* ended in *exedrae* at the church end of the northern and southern arcades. The facade of the westwork of the church is striking for its deep recessed arch, which effectively creates an open triconch in conjunction with the *atrium* (fig. 6.4). A similarity with the so-called palace of the Exarch in Ravenna has frequently been recognised.

The Minster, in essence, grows around a tall central octagonal space, vaulted above clerestory windows, one set in each of the eight walls. A circular aisle and above it a gallery are created around the central octagon and by the sixteen exterior sides of the church. Each of these sides was pierced by two windows, one each for the aisle and gallery. The much larger arches

opening from the first-floor gallery into the central space were embellished by a 'screen' of two sets of double columns, one set above the other. The lower arches of the ground-floor aisle were unencumbered, allowing access to the centre of the church. The eastern end terminated in a simple rectangular choir on the ground floor and above in the gallery. Opposite the choir at the Minster entrance stood a complex westwork, comprising a central tower and flanking round stair towers. The entrance was formed by a porch, above which was the tribune, where the throne was situated. From the tribune a door led to the *porticus* already mentioned above, thus allowing direct sheltered access to the hall. What lay above the tribune is conjectural.

Minster Annex Buildings. From both the aisle and gallery, doors led to the northern and southern annex buildings, both known from excavations of the last century or before the First World War. The northern building was a three-aisled hall, about 23.5 x 15.5 m., preceded by a narthex and ending in an apse. The columns of the aisle were composed of brick. The small hall may have been connected to the *porticus* by an arcade and a further exit on the east side led perhaps into the open courtyard. The southern annex was of the same dimensions and form, although it was quite clearly not aisled. No information is forthcoming concerning entrances except those into the Minster. From the original excavation plans it is clear that the northern building lay measurably (about 1 m.) closer to the Minster than the southern building and that, seen centrally from the west, both buildings were angled away from the observer by a few degrees, thus both deviated from the general alignment of the palace complex in different directions (these deviations are not apparent on Hugot's plan, fig. 6.2). Thordemann believed that the deviation revealed chronological distinctions, but Hugot prefers to doubt the excavator's surveying accuracy by noting that he was two metres out on his measurements between the *aula regia* and the Minster. As this distance was 120 metres, while the smallest gap between the annexes and the Minster was only 2 metres, it seems unlikely that the discrepant distances separating the Minster and its northern and southern wings can be argued away as poor surveying, although the slight deviations from the general palace alignment might be.

There is a question about the exact chronology of the construction of the annexes, although there is no possibility that they were added as an afterthought, for the doors connecting them with the Minster are not insertions and the Minster walls never contained gallery and aisle windows on these two sides as they did on the remaining fourteen. Thordemann (1965, 182) shows that the plan of a royal hall in Pliska near Varna (Bulgaria), dating to the

first half of the ninth century, is almost identical to that of the northern annex, and suggests that it was a royal reception hall, the apse being where the enthroned king would have sat. Thordemann argued that the northern annex pre-dated the Minster and *aula regia* and was Pippin's great hall. It must then be accepted that the Minster was planned, from its inception, to incorporate this royal hall with the addition of the southern wing to balance the composition. It seems on the whole easier to accept that they were all of an original plan, creating what would have appeared like transepts from the outside.

On analogy with Constantinople, the northern building has been called a *metatorium*, where the king could change his vestments or even spend the night on particular religious occasions. An *ecclesia S. Martini cuius basilica sita est in Aquis palatii*, referred to in a document concerning a judgement of 814, was quite probably this building, which was thus the royal chapel. The southern wing is usually identified with the 'hall of the palace called the Lateran', mentioned in the records of an ecclesiastical council held in 817. A synod of 836 called the building of that name a *secretarium* located by the church. The name was, of course, derived from the pope's Lateran palace in Rome. Tradition placed its construction in 796. (Hugot 1965b, 566-7).

The northern and southern wing thus represent the opposition of secular and ecclesiastical authority represented by emperor and pope. To be noted are the throne *exedrae* situated at opposite ends of the two buildings, and this opposition is similarly reflected within the Minster with the throne opposing the altar. The secular side was undoubtedly the northern side, as the emperor was always depicted on Christ's left side, the pope on his right. In the *triclinium* of the papal Lateran in Rome the now lost painting once depicted Pope Leo - St. Peter - Charlemagne. Another interesting opposition then develops. The northern secular building is aisled and thus uses a religious architectural motif while the southern ecclesiastical building was unaisled, using a more secular motif (it will be noted that the *aulae regiae* of Charlemagne at Aachen, Ingelheim, and Paderborn were all unaisled as were the slightly later halls at Bodman and Zürich), for the northern building functioned as the emperor's chapel and fulfilled other of his religious needs while the southern building functioned as an assembly hall for ecclesiastics and thus fulfilled the secular needs of the bishop.

The Baths. The last of the archaeologically detected constructions was the bath complex. As previously noted, the destruction of the Roman baths is dated to 375 AD. Carolingian sources explicitly mention that when King

Pippin bathed at Aachen, it was in natural springs not in baths. A rectangular building of almost the same dimensions (28 x 15 m.) as the gate-hall has been discovered. Three central columns took the roof ridge, and along the southern side the wall opened through four arches, so presumably the building continued in this direction.

Residential Quarters. Finally there are the miscellaneous building which have escaped archaeological detection, although they appear fleetingly in the documentary sources, particularly the domestic quarters for nobles, their retainers, and the numerous servants and artisans of the palace complex. It is tempting to reconstruct a variety of buildings to make up the eastern side of the courtyard (fig. 6.2). As we have seen, at least one construction can be positively presumed south of the *Granusturm*, for in it an original connecting first floor door is still visible. About ten metres from the ground, on the south-east corner of the tower, there is evidence suggesting the previous existence of a roof, allowing the reconstruction of a two-storey adjoining building (fig. 6.3). At several other points some twenty metres east of the *porticus*, sections of walling have been discovered, without enough being present to allow the reconstruction of the buildings' plans. It is conceivable that at the southernmost end of this range there sat the bishop's house.

Einhard tell us (VK 4.32) that the cathedral

was struck by lightning and the golden apple which adorned the highest point on the roof was dashed off by a thunderbolt and thrown on the top of the bishop's house (*supra domum pontificis, quae basilicae contigua erat*), which was next door.

The bishop's house was thus either one of the two annexes discussed above, or it lay to the east of the cathedral. Today this area is occupied by a Gothic choir, which has precluded any excavation. Hugot (1965, 566) notes that the fourteenth-century Gothic addition fits his proposed lay-out modules almost exactly and believes that the choir replaced some building which has fossilised the Carolingian lay-out, possibly even the original Carolingian bishop's house.

Hugot suggests that Charlemagne's residence would have been near his beloved baths and thus suggests somewhere between Minster, *aula*, and baths. I believe otherwise. Oppositions of ecclesiastical and secular buildings have already been seen quite clearly at Aachen and if continued, and if the bishop's house be taken to lie under the Gothic choir, we might postulate that Charlemagne's residence was to be found at the northern end of the eastern courtyard range. Charlemagne's domestic quarters were perhaps actually within the *aula regia*. Einhard (VK 4.32) claimed that 'there were frequent earth

tremors in the palace at Aachen, and in the apartments where Charlemagne lived the wooden beams of the ceiling kept on creaking (*in domibus ubi conversabatur adsiduus laqueariorum crepitus*).’ Hugot (1965, 545) claims that the beams in stone buildings do not creak, therefore the residence was in timber. This can be rejected. Not only were all the archaeologically detected buildings in stone, but the royal apartments at Ingelheim were of stone, and to expect less of Aachen cannot be right. The other documentary reference to Charlemagne’s apartments, which includes those of the others in his court, is that of Notker (*De Gestae Caroli Magni* 1.30):

mansions belonging to men of various rank were erected around the palace of Charlemagne in such a way that, shrewd as he was, through the windows of his private apartment, he could see everything they were doing, and all their comings and goings, without their realizing it. In the same way all the houses of his nobles were built high off the ground, so that the retainers of the nobles, the personal servants of those retainers and every other passer-by could be protected from rain or snow, cold or heat, and yet the nobles themselves could not hide from the eyes of the ever-vigilant Charlemagne.

As Notker ends the passage by confessing – something we easily gather from studying his text – that he was in effect shut up in his monastery in St. Gallen and had not been to Aachen, we must not take his testimony as reliable.

Bibliography: Buchkremer 1949, Christ 1958, Cüppers 1982, Falkenstein 1966, Fichtenau 1951, Hugot 1965a, 1965b, Schlesinger 1968, Thordeman 1964.

Ingelheim (Rheinland-Pfalz). Ingelheim is located on a north facing slope, three kilometres south of the Rhine and a dozen west of Mainz. The setting thus much resembles the ideal setting of a Roman villa; sheltered from the wind avoiding the top of the hill, commanding a view of a main river and in close proximity to fresh water, in this case the Selz stream lies 1.5 km. away, but fresh water is easily obtained by wells in Ingelheim where the water table is high. Communication with Ingelheim was excellent: either by ship along the Rhine or by road towards Mainz or Boppard.

It should come as no surprise from what has been said above that the possible remains of a Roman villa were in fact found somewhat to the west of St. Remigius’ church, although recent excavation has disproved the old belief that a Roman villa had actually existed within Ingelheim, or under the Carolingian palace, but the discovery of reused Roman funerary stones points to some nearby occupation. The only archaeologically detected predecessors of the Carolingian villa are *Reihengräber*. Weidemann recreates six separate cemeteries and concomitant farmsteads (fig. 6.5), although some of the

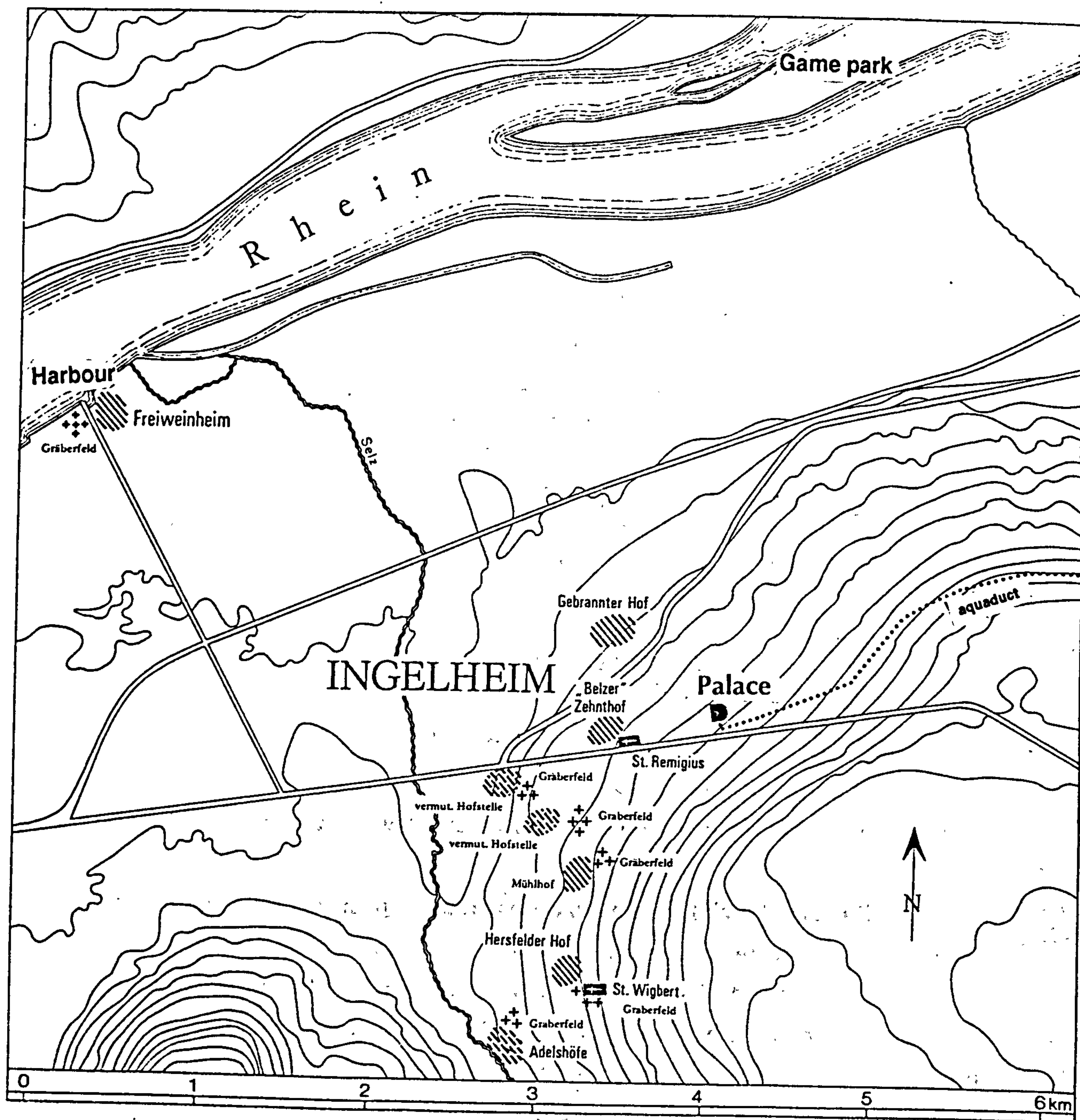


Fig. 6.5 Local area around Ingelheim palace, showing the location of Merovingian burials, recorded Carolingian churches, and the (very) hypothetical position of Merovingian estates (Höfe). The port and the game park are the assumed locations of those mentioned sites in the Poem to Louis the Pious (after K. Weidemann).

'cemeteries' consist of no more than a few excavated graves. The largest, sixty graves found in the field of the revealing name 'Im Totenweg', dates to the sixth century. All the remaining 'cemetery' sites date to the seventh, with the exception of the Freiweinheim site, thought to pinpoint the harbour site for Ingelheim, which dates to the eighth century. Thus rather than Weidemann's

multiplicity of farmstead sites, one might accept two or perhaps as many as three for the seventh century. Field names north of St. Remigius' church and downhill from the Carolingian palace site are reckoned by Weidemann to be the main villa farmstead site which predated the palace. The church was one of 25 churches and chapels gifted by Carloman in 741/3 to the bishopric of Würzburg on the occasion of its foundation. Charlemagne donated property further south in Ingelheim to Hersfeld monastery on which was later to be built St. Wigibert's monastery. It thus appears that land from one end of Ingelheim to the other was in the gift of the Carolingians and we can clearly consider the whole area as a royal estate.

In 774, according to the *RFA*, Charlemagne sent four detachments of the army into Saxony on his arrival at Ingelheim, returning from a victorious campaign against the Lombards. It need not be that any royal domestic residences existed at this time, for Charlemagne was in essence on campaign, and there is no suggestion that he did any more than stay the night at what may have been no more than one of his agricultural estates. From 787 onwards, however, it is clear that a palace existed at Ingelheim. Einhard mentions in his *Vita Caroli* (2.17) the construction of two magnificent palaces, one near Ingelheim and the other at Nijmegen, in a chapter devoted to Charlemagne's building projects. It is clear by the description *iuxta* that the palace was constructed near the existing villa, which is presumed to lie near St. Remigius's church some 500 metres away. From excavations only one timbered hall and a *Grubenhaus* of the seventh century have been recognised under the palace, just as we might expect if the palace was built *by* Ingelheim. If, as seems plausible, the Carolingian property was formerly Merovingian, we know where the villa probably lay, but have no idea what it might have looked like.

Excavation has, on the other hand, given us a very good idea of what the palace, as built for Charlemagne and Louis the Pious, looked like. Excavation at Ingelheim has been extensive. Those of C. Rauch between 1909 and 1914 are perhaps the most famous, although not the first and only recently published. Various further excavations were undertaken in the 1960s, directed successively by W. Sage, H. Ament, and U. Weimann. These recent excavations have been helpful in better dating the components of the complex rather than extending the known plan. Fig. 6.6 represents the total excavated plan from the various excavations. With the exception of the later Ottonian church set in the middle of the south side, the rest is probably entirely Carolingian, although there is some doubt about the buildings opposite the Ottonian church within

the courtyard.¹

The Hall. The so-called *aula regia* consists of a 33.2 x 14.5 m. hall with a round apse of 4.9 m. radius attached to the south end. In the east and west long sides there were doors 2 m. wide and 4.2 m. high. Although Rauch originally thought the hall to contain a double row of columns, thus producing a basilican effect (fig. 6.7a), subsequent excavation has rejected this interpretation. The hall is now seen as being unaisled. The position of the row of windows some 7 m. up from the base of the wall is probably attested in one instance in the remaining fabric of the south-western corner of the hall, according to Weidemann. Sage (1974), however, publishes an almost identical figure of the extant wall, but not quite to the height of the base of the window, which is portrayed as hypothetical. The apse had four windows, considerably lower than those along the main wall, three of which still survived when Rauch investigated the hall. The red sandstone was set in a pure white mortar, similar to that found at Frankfurt, but quite different to the red mortar of Aachen, although many publications before the 1960s state otherwise. The walls were probably painted on the inside, for fragments of red and brown painted plaster that surely derived from the walls are found between the two floor levels, the latter attributed to the Ottonian period (a dendro-date suggests a thorough remodelling ca. AD 970). Other debris included fragments of red tile, almost undoubtedly deriving from the Carolingian roof. Less clear are the extensions to the north of the hall. Since Rauch's excavation, this area has not been reinvestigated, but all subsequent investigators have accepted Rauch's dating of the buildings as Carolingian. Rauch had originally conceived of the extension as buildings clearly distinguished from the hall (partially visible in fig. 6.7a). A set of triple arches, two at least found in their entirety, were interpreted as forming something like the still upstanding Lorsch monastic gate-hall (fig. 6.6). Jacobi, on the other hand, reconstructs the entire west wing as one continuous building. However, the arches seem out of place as an internal feature.

The Exedra. Recent excavations in the eastern end of the palace, in the architecturally unique *exedra*, have largely vindicated Rauch's earlier interpretations. An otherwise square palace courtyard complex ended in a

¹ In the excavation report of the Northampton Saxon palaces (Williams 1985) one finds comparative plans of Carolingian palaces (all of which are discussed in this chapter). In the report, the plan of Ingelheim follows the interpretations of Rauch from the beginning of this century (and thus the plan given is essentially that of the reconstruction of fig. 7.7a). It is therefore wrong on many counts, as will become clear in the course of this section on Ingelheim. To name but three striking mistakes, the Ottonian church is included in the Carolingian plan, the towers are absent, and the hall is given aisles.

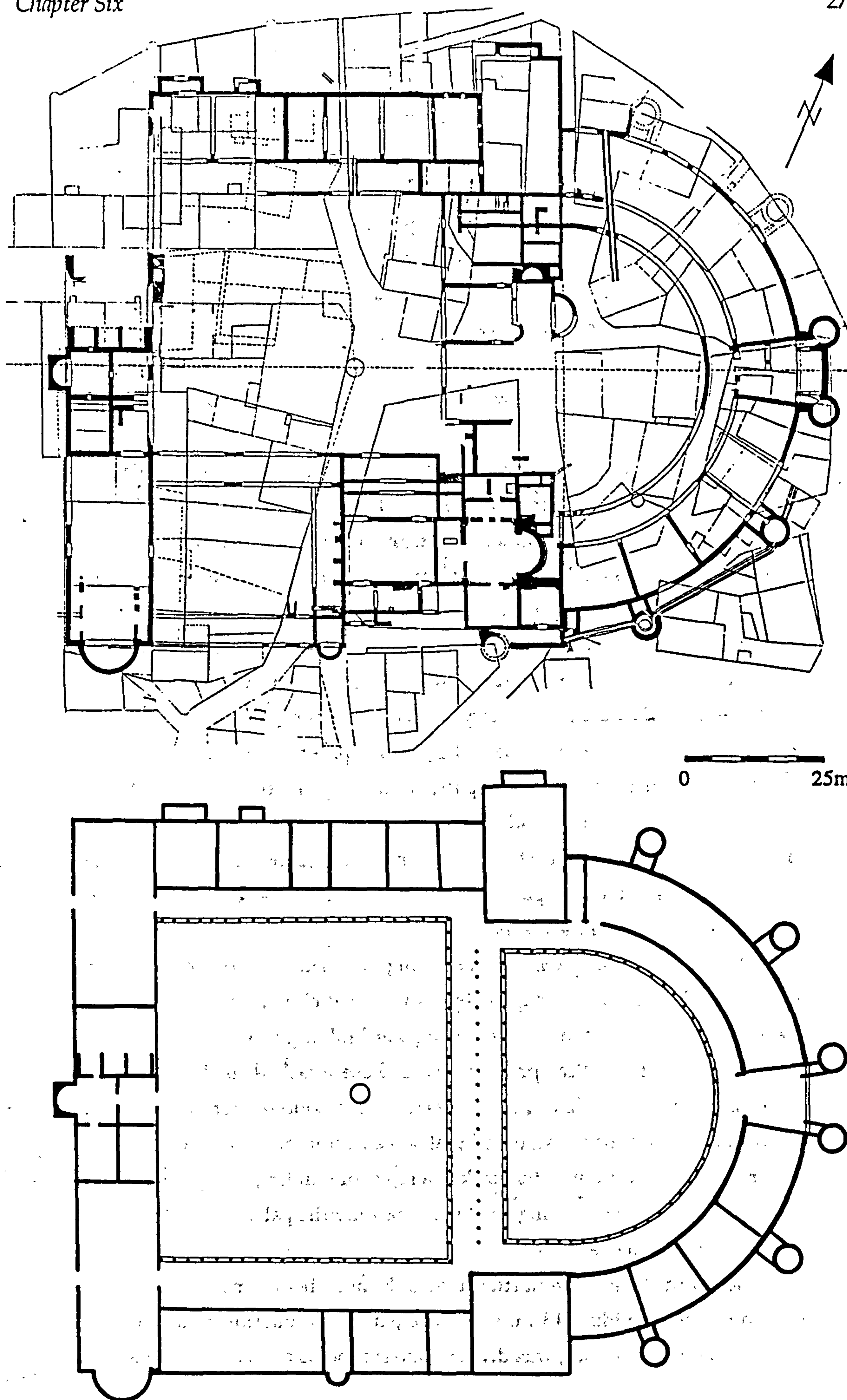


Fig. 6.6 The Carolingian and Ottonian palace remains at Ingelheim from Rauch's excavation and Jacobi's reconstruction of the ideal Carolingian plan.

curved continuous building, the outside wall of which formed a semicircle 90 metres in diameter (the side of the building on the inner side of the courtyard was 62 m. in diameter.). The curved rooms thus formed were just under 11 m. wide and interior divisions found in the southern half of the *exedra* produced three equal-sized rooms of 10-13.4 m. in width and a remaining space somewhat larger (14.9-19 m.) although how or if it was subdivided is unknown. Around the inside of the *exedra* ran an arcade, which would have allowed a usable passage width of 5 metres. A reconstruction of a column in the *porticus* from fragments reveals an impressive piece of architecture and reveals clearly the Roman inspiration well known from pieces of church architecture: an inspiration which was part and parcel of the so-called Carolingian renaissance.

Central to the *exedra* was the main entrance to the palace. In reconstruction (figs. 6.7 and 6.8), a gate-tower is traditionally considered appropriate, although the foundations here are neither wider nor deeper than elsewhere in the *exedra*. Later Romanesque additions to Ingelheim included the addition of a battlemented entrance between two flanking circular towers. Rauch was led to conclude that the towers were likewise Romanesque, but excavations in the 1960s proved that the five towers detected (and one is thus postulated to balance the symmetry) would have been originally integral parts of the palace. They were connected by two short stretches of walling to the *exedra* which must have housed a passage, although no traces were detectable at ground level. A water conduit led, presumably from a water source some many kilometres to the east, through the foundations of two towers and eventually passed into the palace proper, where it can be seen running under the northern section of the *exedra*.

The South Range. Of the remaining two sides of the complex, the southern side was later occupied by an Ottonian church. At its eastern end there seems to have been a large rectangular building to which was added a circular tower of identical proportions to those attached to the *exedra*. This tower lay just inside a line drawn directly to the south-eastern corner of the royal hall. It is commonly assumed that some continuous barrier existed here, either as a simple wall or as the back of a range of buildings. A very small apse lies along this line, protruding, as it were, beyond the palace wall. Its purpose may have been as a chapel.

The North Range. The northern range is much better known. It consisted of a continuous building 13 m. wide, fronted on the interior by a 6 m. wide *porticus*. The long building was divided into six rooms of sizes varying around

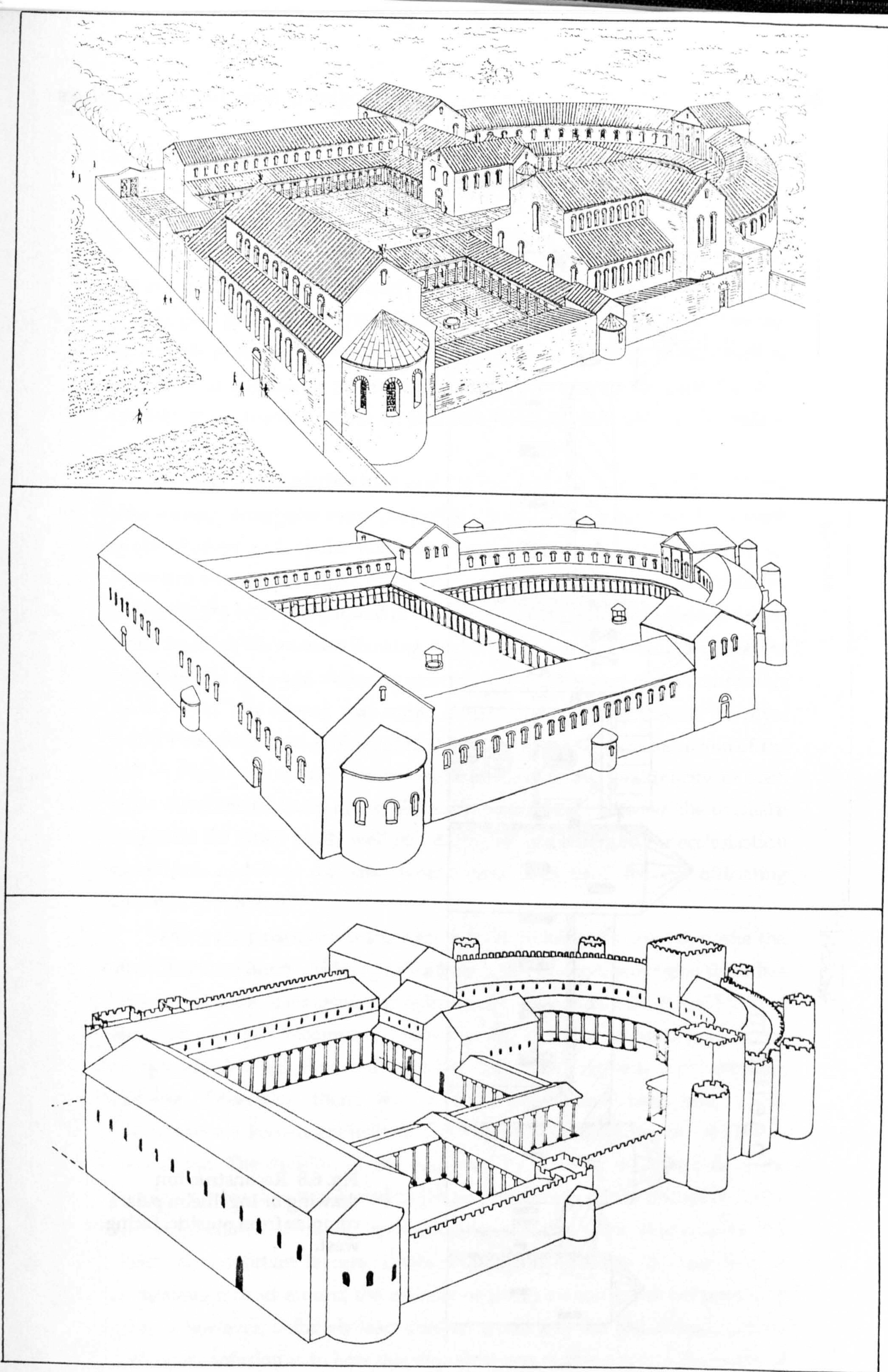


Fig. 6.7 Three reconstructions of Carolingian Ingelheim seen from the south. Top: by Franz Krause based on Rauch's interpretation (note the inclusion of the Ottonian church); middle: by Jacobi; bottom: by Ross Samson based on a model made to the specifications of Kurt Böhner (note the odd gap created by the removal of the Ottonian church).

Ingelheim

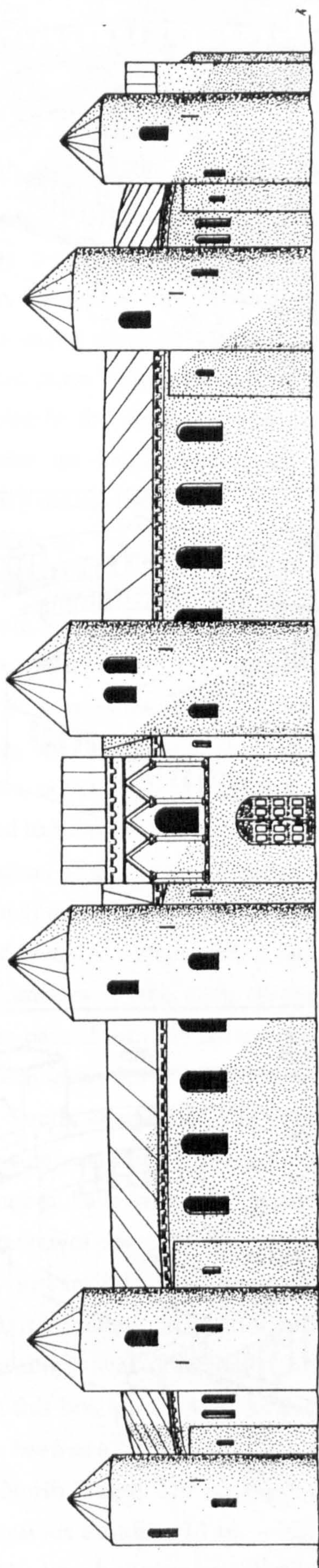


Fig. 6.8 Reconstruction drawing of Ingelheim palace complex from outside, facing west.

an average of 8 m. In the north-western end two small rectangular rooms projected at most 2.5 m. from the exterior facade, along which no traces of more circular towers were found.

The north-western corner of the complex is often left incomplete in reconstruction drawings, because no excavation has been carried out here. A good reason for suspecting that, unlike Jacobi's reconstruction, there was no continuous building, but rather a closure wall with gateway, is that there is otherwise no good access into the palace complex for horses and carts. Rauch's original reconstruction offers a plausible 'service entrance' to the palace complex.

Between this northern range and the *exedra* was a large hall, 25 x 16 m., with a small rectangular room projecting outside, just as was found in rooms at the western end of the northern range. Weidemann would have these reconstructed as towers (fig. 6.7c), although it may be better to see them as simpler forms of the apse found in the royal hall. Semi-circular recesses, apses, were found on the annexes flanking Aachen cathedral and were interpreted as niches for the king and bishop respectively. It will be seen that a comparable small recess, only square, was added to the building interpreted as the royal hall at Paderborn, thus analogous to the round apse at the southern end of the hall in Ingelheim. Clearly there seems an unnecessary multiplicity of such apses along the northern range, although the large hall between the northern range and the *exedra* might well be interpreted as a hall used for ecclesiastical assemblies, and thus the apse would have been used for the officiating archbishop or bishop.

Within the palace courtyard were several buildings situated opposite the later Ottonian church. Although Rauch took them to be Carolingian there has been no subsequent excavation to substantiate or invalidate the claim. Much of the plan is very incomplete, allowing an apse-like construction to be recognised, although divorced from the rest of the originally accompanying building. Otherwise there was a bath house and bath and, given Charlemagne's known enthusiasm for bathing, it is tempting to see this as Carolingian. The division of the courtyard by covered walkways depends largely on what date is assigned to the various constructions (including bath-house and church). Fronting the north range and the *exedra* were arcades and clearly an important feature. Jacobi would prefer simply to reconstruct a continuous *porticus* around the entirety of the inner courtyard. Sections of a *porticus*, however, definitely lead from an entrance to the hall, although there is distinct confusion as to how this should be seen continuing into the centre of

the courtyard (see different reconstruction attempts in fig. 6.7).

Bibliography: Lammers 1973, Rauch and Jacobi 1979, Sage 1976, Schmitz 1974, 1976.

Paderborn (Nordrhein-Westfalen). Paderborn lies in the south-east corner of the Westphalian forest. Long distance routes run west from Köln over the Grosser Hellweg east to the passes of Egge and through the Teutoburger Forest towards Magdeburg. Routes also run south towards Frankfurt through the Wetterau (Hessen). The east-west route was the most important to Paderborn and this was the route taken in 753 when King Pippin fought the Saxons.

Excavation immediately to the north of the cathedral in the 1960s revealed the early medieval royal palace complex. The site can be conveniently divided chronologically by the great fire of 1000 AD. Evidence of the fire is abundant and necessitated complete reconstruction. Underlying this Ottonian complex was the Carolingian royal palace and cathedral of at least four phases, although each successive phase largely maintained the plan of the previous. There is evidence of three construction phases of the main hall and the buildings lying to the north of it. Each reconstruction was clearly necessitated by fire damage: the stonework surviving from previous phases was often fire reddened. Rebuilding even began as far down as the top of the foundations. A composite plan is all that is offered in the excavator's easily accessible reports. Separation into phases is left largely to the reader, although the sorting out of the different phases around the hall is aided by a plan offered by Gabriel (1986) (fig. 6.10) based on Winkelmann's confusing text. The dates included for the construction of each phase have been derived from the documentary evidence concerning Saxon attacks – to which the fire damage is generally attributed.

Paderborn was first mentioned in 777 when, according to the *RFA*, Charlemagne held his first assembly there. Three monastic chronicles all record the construction of a church dedicated to St. Salvator in this year and the holding of a synod. In 778 there was a destructive Saxon uprising, during which it is supposed that Paderborn was destroyed. A second destruction period is suggested for the year 794, for Saxons were defeated at the Sintfeld, just south of Paderborn, according to the *RFA*. In 799 building seems to have taken place in preparation for the arrival of Pope Leo III, whose meeting with Charlemagne was commemorated by the poem *Epos Karolus magnus et Leo papa*.

The palace complex, as recovered archaeologically, was composed of: a hall, to the south of which a courtyard grew as new ranges were added to the

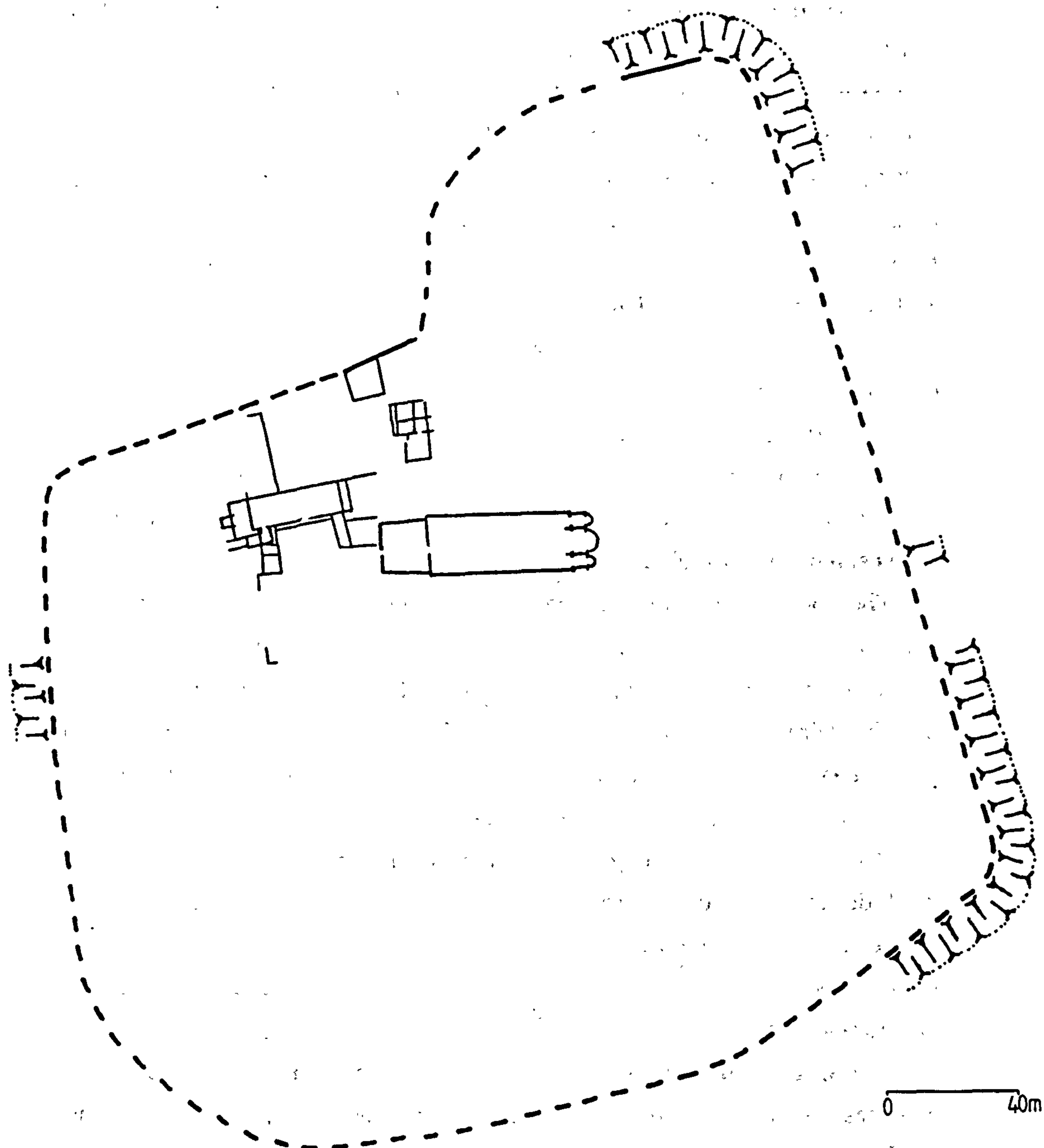


Fig. 6.9 Plan of the earliest phase of the Paderborn palace complex and the circuit of the rampart, showing excavated stretches and hypothetical line following the boundary of the later episcopal immunity (redrawn after Winkelmann).

eastern and western end of it; a church; annex buildings assumed to be residential; and a rampart with tower(s). Undoubtedly much more existed, if only because the complex as it is known occupies only a small portion of the area within the walls (fig. 6.9).

The Hall. The hall, 10.3 x 30.9 m., was built of .65 m. (2 Carolingian feet) wide mortared walls of quarried limestone and roughly shaped red sandstone blocks for the corners. The mortar was of burnt lime with a generous admixture of fragments of red brick, thus resembling the mortar used at Aachen. Fragments of painted plaster have been found in the building. The hall was never aisled and began as a large hall with small chambers at either end; that in the eastern end was seemingly a latrine. Entrance was gained through a small stair tower. Later additions to the west of the hall included an additional room with thicker walls and a rectangular recess, thought to be a niche for the throne. The stair-tower was also rebuilt, although its exact form is difficult to disentangle from the information provided by the excavator. Along the southern wall of the hall, a parallel wall was added. The excavator thinks it was added for strength, but the gap might conceivably have contained a narrow arcaded gallery, although at little more than a metre in width, it would have been uncomfortably narrow.

The documentary sources, the *Epos* excepted, say nothing about profane Carolingian buildings in Paderborn (Balzer, 1979, 50). The poem does record that after the pope's arrival, Charlemagne and Leo went on foot up to the church where the pope celebrated mass. Afterwards the pope was invited to take a meal with Charlemagne in the royal hall (*regalem tendit ad aulam*)(*Epos* 5.433). Here there were wall tapestries (*clara intus pictis conlucet vestibus aula*)(*Epos* 5.533). Charlemagne ascended his throne to give judgements, conclude treaties, and even to call the great men to an *occursus* for the pope (*ipse sedet solio Karolus rex iustus in alto*)(*Rex pius interea solium conscendit*)(*Epos* 5.449 and 463). The description fits the picture of the hall and shows it was used for banquets.

The Courtyard. The growth of the courtyard and its surrounding buildings is very confused. Gabriel offers his interpretation of the development (fig. 6.10), although the composite picture cannot represent the final state of the complex. Thus, for instance, the stippled areas that represent open yards could not have existed simultaneously as depicted. Furthest from the hall is the *atrium* of the earliest church, and presumably the arcade leaving the hall originally led to that *atrium*. The construction in figure 6.10 marked AD 836 is in fact the northern end of a western transept, a later addition to a church that had already replaced and overlay the earliest church and its *atrium*. By the time this transept was built, and the main palace courtyard was formed (stippled) the *atrium* was already out of use. With the building of the transept, the portico extending from the eastern end of the south side of the hall

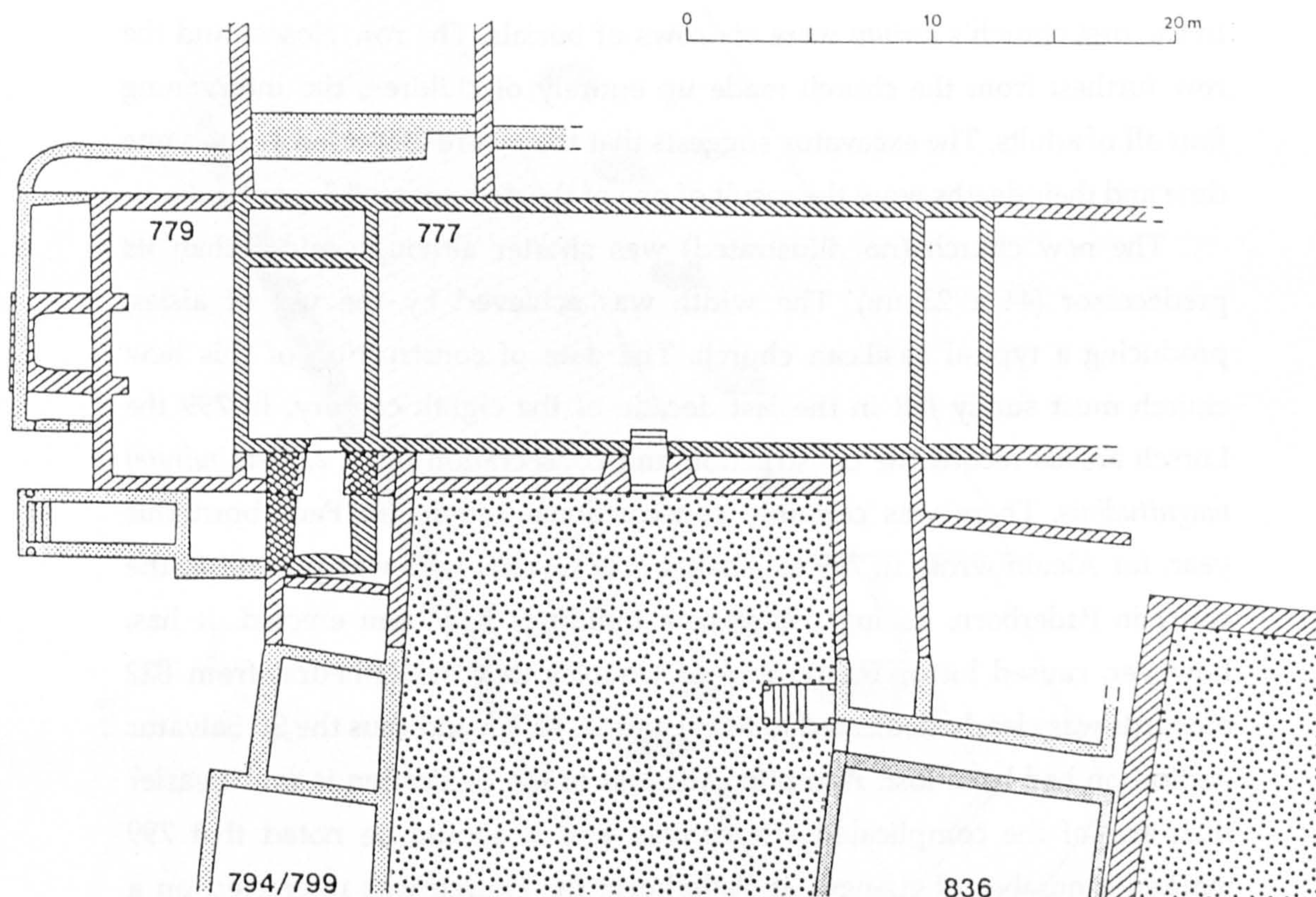


Fig. 7.10 The palace courtyard with a tentative dating of different phases although all could not have stood simultaneously (after Gabriel).

presumably lost its sense, unless the continuous foundations hide from us the existence of a private doorway into the church at this point. The plausibility of this interpretation is strengthened by the existence of a later stretch of wall running parallel to the transept wall. It apparently forms a little access-way or passage around the northern side of the church. The wall can, whatever its function, only make sense if the arcade were still standing.

The main palace courtyard was only formed when buildings were added to the west. Within the courtyard now created, a small structure, thought to be a base for a throne, is to be found. Five steps lead to a small plinth, the entirety of which was covered by a baldachin supported on four posts.

The Church. Little evidence survives of the earliest church constructed in 777, for it was badly mutilated by a post-war oil tank. It appears to have been an aisleless hall, 52 x 18 m., terminating in three apses. Following a fire it was rebuilt along the same lines, the floor level slightly raised and an *atrium* added. There does not appear to have been a third rebuild as in the case of the hall, so that this phase would appear to be represented by the building of a new church under the present cathedral. Excavation from 1978 to 1980 revealed that

the new Carolingian cathedral was built over the cemetery of the first church. In the first church's *atrium* were six rows of burials. The row closest and the row furthest from the church made up entirely of children, the intervening four all of adults. The excavator suggests that they were all buried at the same time and their deaths were the result of one of the documented Saxon raids.

The new church (not illustrated) was shorter although wider than its predecessor (44 x 23 m.). The width was achieved by the use of aisles, producing a typical basilican church. The date of construction of this new church must surely fall in the last decade of the eighth century. In 799 the Lorsch annals record the construction and consecration of an *ecclesia mira(e) magnitudinis*. There was certainly constructional activity in Paderborn this year, for Alcuin wrote in 799 to Adalhard of Corbie, who was present at the court in Paderborn, asking if any new buildings had been erected. It has, however, caused historians no end of troubles that the cathedral from 822 onwards was clearly dedicated to Mary and St. Kilian and thus the St. Salvator dedication had been lost. Accepting the loss of the dedication is much easier than any of the complicated alternatives, and it should be noted that 799 marked a number of changes at Paderborn; the church was re-erected on a different spot (it had moved the width of the church to the south), the time gap separating the two may have been a good number of years, and most significantly Paderborn was raised to the dignity of a bishopric in this year. To this new church was later added a 34.5 m. wide western transept and western *atrium* (just visible in fig. 6.10). The addition is thought to be the result of bishop Badurad's work, culminating in the translation of St. Liborius' bones in 836. Some time in the early ninth century there was the growth of a monastic complex to the north of the church. (Refs. in Winkelmann 1972, 204-211).

Residential Buildings. The last buildings to be considered in the palace complex are the small, probably single-storeyed buildings north of the *aula regia* and church. These were carefully constructed and as in the case of the hall and church, a great deal of painted plaster was found inside these buildings. Reconstruction following the first fire resulted in rebuilding with an altered plan, which accounts for the confusing plan in Winkelmann's reports.

According to the *Epos* (5.533), following the meal in the hall the pope returned to his entourage while the king retired to his secret hall (*aule secreta revisat rex*). The court and pope would appear to have been lodged in buildings referred to as *tecta* on three occasions and once as *sedes . . . ad culmina* (*Epos* 5.432, 523, 527, 512).

The Enclosure. The Carolingian rampart enclosing Paderborn has been

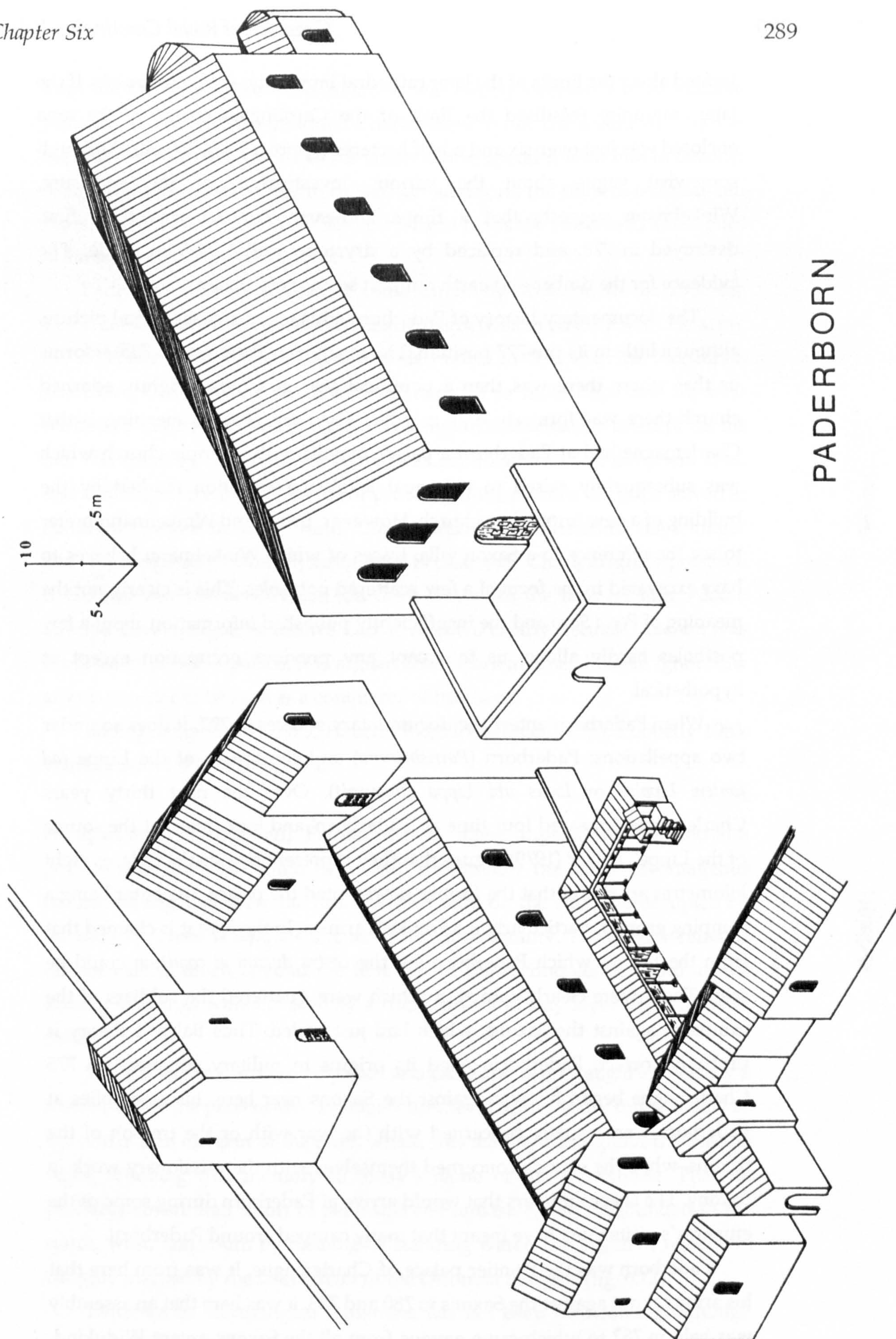


Fig. 6.11 Reconstruction in axonometric view of an early phase of the palace complex.

located along the limits of the later cathedral immunity, at several points. If the later immunity fossilised the limit of the Carolingian enclosure the area enclosed was just over six and a half hectares (approx. 280 x 250 m.). Although somewhat vague about the various investigation of the enclosure, Winkelmann suggests that a timber-and-earth rampart was built first, destroyed in 778, and replaced by a drystone wall 1.3-1.5 m. wide. The evidence for the timber-and-earth rampart seems very unclear.

The documentary history of Paderborn adds to the archaeological picture, although little to its pre-777 position. The much later *Poeta Saxo* (1.335) informs us that where there was then a pontifical seat in a most brightly adorned church there was formerly only a 'villa'. The very obvious meaning is that Charlemagne had at Paderborn a palace or villa with a simple church which was subsequently raised to episcopal level, the elevation marked by the building of a new impressive church. However, Balzer and Winkelmann prefer to see the reference to a Saxon villa, traces of which Winkelmann believes to have excavated in the form of a few scattered postholes. This is clearly not the meaning of *Poeta Saxo* and the insufficiently published information about a few postholes hardly allows us to accept any previous occupation except as hypothetical.

When Paderborn enters the documentary sources in 777, it does so under two appellations: Paderborn (*Patrisbrunna*) and the source of the Lippe (*ad fontem Lippiae* or *locus ubi Lippha consurgit*). Over the next thirty years Charlemagne appeared four times at 'Paderborn' and four times 'at the source of the Lippe'. Balzer (1979) argues that they represent two distinct places eight kilometres apart and that the former represented the palace, the latter being a camping ground, particularly for Frankish armies. In the *Epos* it is claimed that from the hill on which Paderborn lay the *castra ducum et comitum* could be seen. These were clearly camps in which were quartered the soldiers of the campaign against the Saxons, which had just ended. Thus Balzer's theory is probably correct. Paderborn owed its origins to military conquest. In 775 Charlemagne began his war against the Saxons near here, his assemblies at Paderborn were usually concerned with the war with or the treason of the Saxons, while the synods concerned themselves with the missionary work in Saxony. The sheer numbers that would arrive at Paderborn during some of the emperor's visits must have meant that many camped around Paderborn.

Paderborn was the frontier palace of Charlemagne. It was from here that his army set out against the Saxons in 780 and 783, it was here that an assembly was held in 782 to which came envoys from all the Saxons except Widukind,

from the Danes, and from the Avars. It was clearly deemed a most suitable place for assemblies, for although Charlemagne spent the winter of 784/5 at Eresburg and had even brought his family out to spend it with him in Saxony, he called the Easter assembly to be held at Paderborn, the third in less than ten years. Further assemblies were held in 804 and Louis the Pious held assemblies in Paderborn in 815, 840, and 845.

Paderborn's greatest moment came rather by accident. In the 790s the Saxon conflict flared again, so that Charlemagne was in Saxony with his army in 799. While his eldest son Charles headed north to deal with the Northmen and Slavs, Charlemagne remained at Paderborn to meet Pope Leo III, who wished to meet with Charlemagne following an assassination attempt on his life. The meeting, as mentioned above, was commemorated in the poem *Epos*, and similarly in the *Life of Leo III*. For probably three months the pope stayed at Paderborn before heading south. Charlemagne remained some days longer and received envoys from Byzantium. It is clear that Charlemagne's presence in Saxony was not needed; his son dealt ably with the task set him. Alcuin advised Charlemagne to receive Leo at Aachen, clearly because Aachen was his most impressive palace. It is argued that Charlemagne chose to ignore the advice in order to be seen as a conqueror of heathens.

Bibliography: Balzer 1979, Hauck 1968, Honselmann 1980, Lobbedy 1983, Winkelmann 1971a, 1971b, 1972.

Frankfurt am Main (Hessen). Frankfurt, as its name implies, was situated at a point where the Main could be forded. Presumably the same advantageous natural setting also drew the Romans who built on the site now occupied by the old city. There is no question, however, of continuity. The datable remains do not extend much beyond the first half of the second century and all the Roman buildings were covered by a thick earth and clay layer up to a metre thick.

The site of the Carolingian palace and cathedral is on a slight rise nearly 8 metres above the present-day average water level of the Main. Immediately to the north is an old arm of the river, which was marshy throughout the Middle Ages, retaining that memory in today's name of Braubachstraße. The site probably constituted a sort of peninsula in Frankish times, surrounded to the north, west, and south by running or standing water, the edges of which are roughly marked by the later circuit of the Ottonian rampart (fig. 6.12).

Evidence of Merovingian settlement has not been forthcoming although some sherds of pottery have been found that might date to the period. At

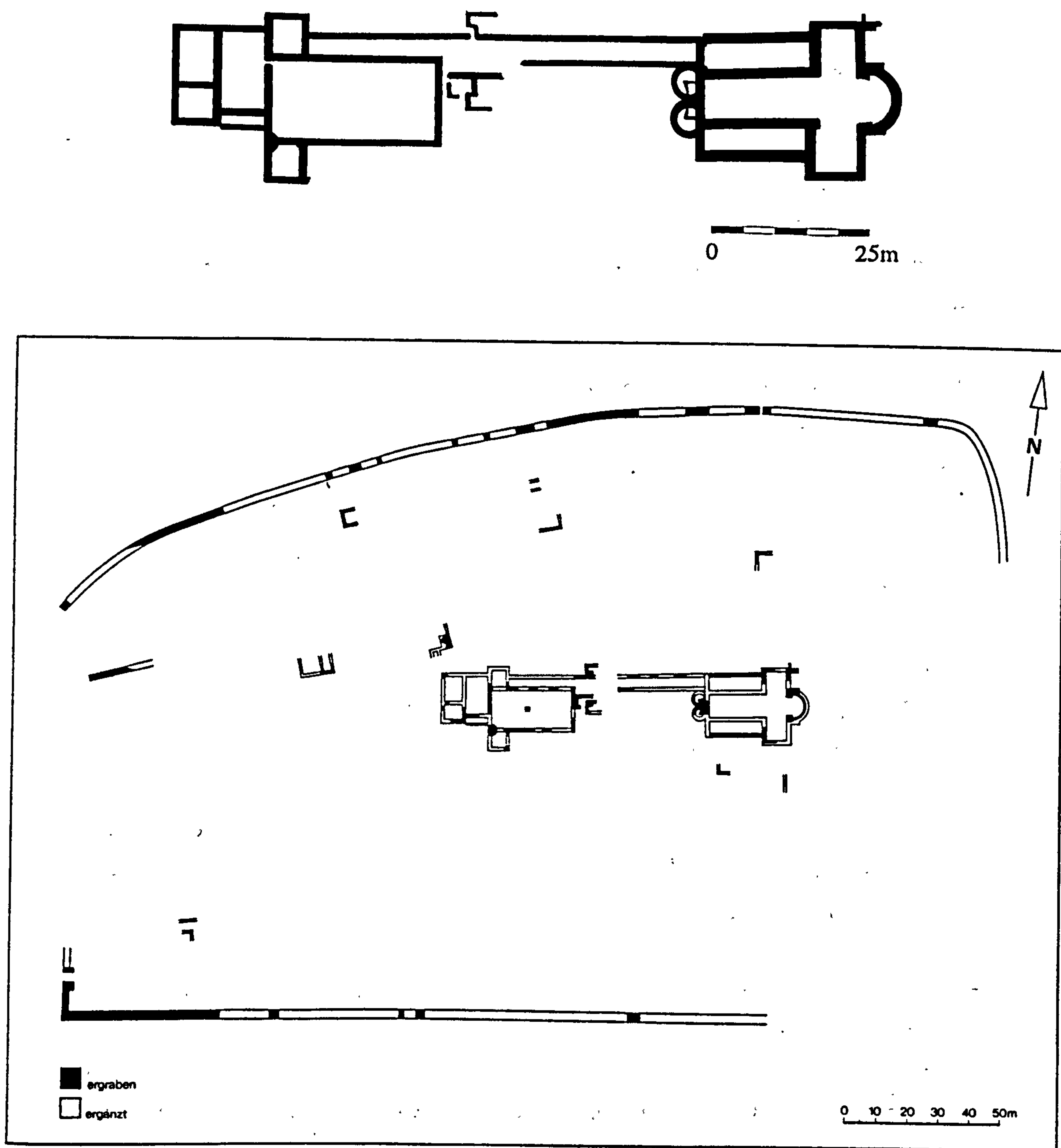


Fig. 6.12 Plan of the Carolingian palace at Frankfurt (after Hundt, Fischer, and Stamm).

present, a new foundation in the Carolingian period seems most likely to my mind, given what I have argued for in the last chapter about the lack of estates beyond the Rhine before the Carolingian period. The secondary Germany

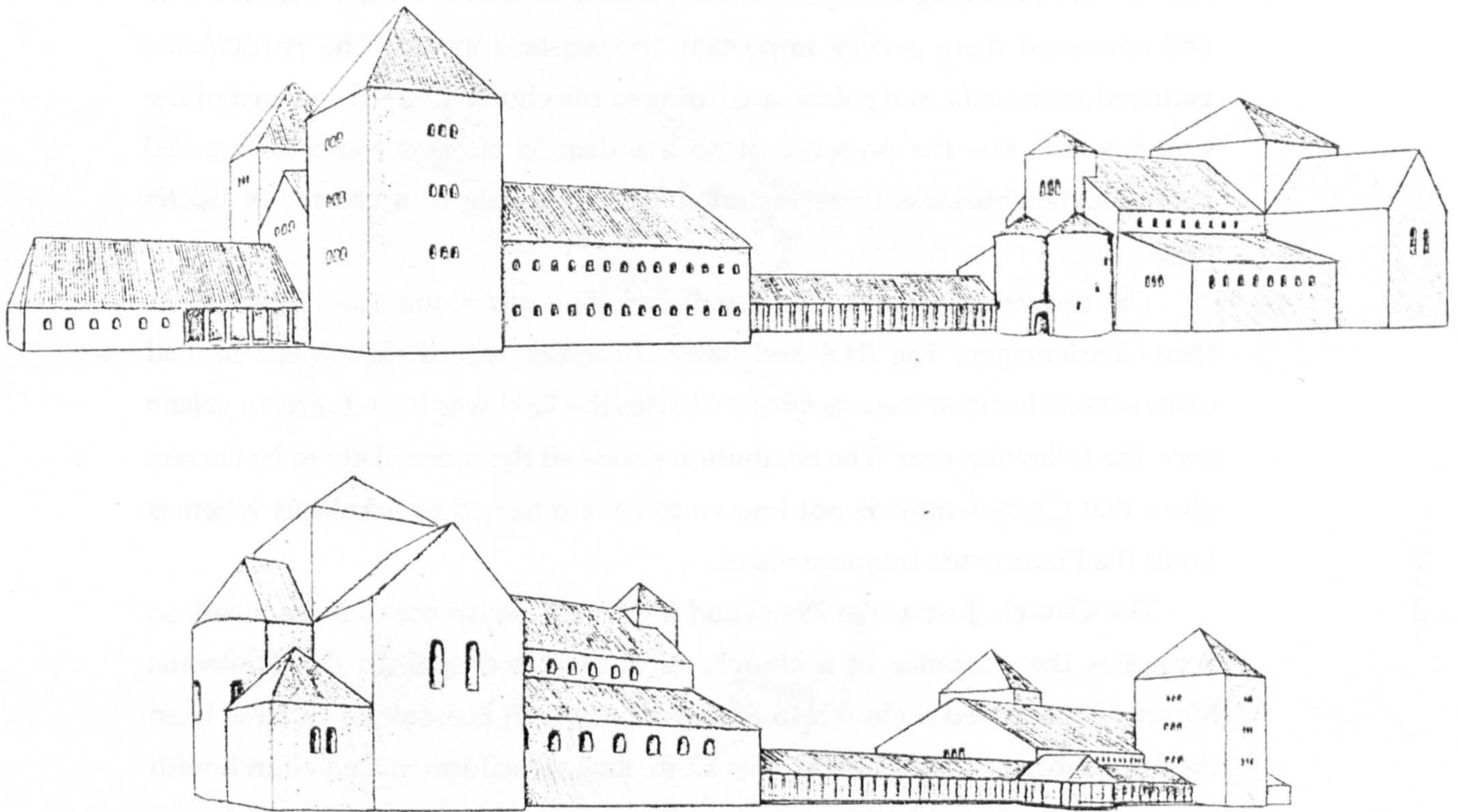


Fig. 6.13 Reconstruction of the Frankfurt palace and cathedral (drawing by H. Ribbeck).

literature, as ever, assumes that a Merovingian estate was the predecessor.

The Hall. The large, 12.2 x 26.5 m., rectangular hall had a central line of column supports, making it an uncommon example of a two-aisled hall. The excavators believed it held a second storey. Joined to the hall at its western end were three rooms. A passage-way, like the *porticus* at Aachen, joined the hall to the church and is presumed to have continued along the northern face of the hall, although it was not found there. The small room to the south of the hall has been interpreted as an antechamber; one is mentioned early in the tenth century. I would prefer to see the passage-way/*porticus* as marking the front entrance, as it does at Aachen, Ingelheim, and Paderborn, and thus one of the western chambers would have acted as the fore-hall. This is perhaps all the more likely given that the western chambers may have formed a sort of *Westwerk* to the *aula regia*, just as the church was apparently flanked by western towers.

Construction, as it was throughout the Carolingian complex, was of basalt foundations with Roman *spolia*, and quarried basalt and sandstone walls; the stone was bound with hard white mortar.

The first documented reference to the palace is in the *RFA*, which record that in 794 Charlemagne stayed in *villa Franconovurd* over winter with his wife and convened there a very important ecclesiastical synod. The participants gathered in the *aula sacri palatii*, according to the church council's record of the synod, which saw the presence of no less than 38 bishops and a further 300 people. Other business there included the witnessing of a charter in *palatio regio*.

The excavated royal hall is usually attributed to Louis the Pious, rather than Charlemagne. The *RFA* and other chronicles record that in 822 he had *constructis ad hoc opere novo aedificiis*. Charles the Bald was born there, in *palatio novo*, the following year. The attribution seems all the more likely to be correct given that Charlemagne is not known to have returned to Frankfurt whereas Louis the Pious made frequent visits.

The Church. Just as the 794 synod implies the existence of a great hall, so it implies the existence of a church, or at least a chapel. In 852 Hrabanus Maurus consecrated a church to St. Salvator, which is assumed to have been the forerunner to the cathedral. The 32 m. long, cruciform, aisled church with two round western towers perhaps with central stair towers, found during excavation (fig. 6.12) was probably built in the reign of Louis the German. The contemporary chronicler at St. Gallen recorded that he had *oratoria nova . . . admirabili opere construxit* at Frankfurt and Regensburg. At Regensburg, Niedermünster II is very similar in size and plan and almost definitely was the work of Louis the German's reign.

Bibliography: Hundt and Fischer 1958; Stamm 1955 (all textual refs. in Stamm).

Bodman (Baden-Württemberg). Situated on the bank at the northern end of Überlingensee, an extension of the Bodensee (Lake Constance), Bodman has yielded some few Merovingian-aged burials. Historians have attempted to locate here a royal estate at the time of the foundation of Reichenau ca. AD 724, but no positive evidence of a royal villa exists before the early ninth century, when it was once visited by Louis the Pious.

Excavation has yielded one large building, 38.8 x 13.8 m., with stone walls some 2.8 m. wide. It is almost beyond doubt that such thick walls were designed to carry more than one storey (fig. 6.14).

No traces of fortifications have been found around the building, but it is set on a platform which falls steeply down to the lakeside. There was, however, an enclosure around the church which must have sat under the present day church, around which a few appropriately dated burials are

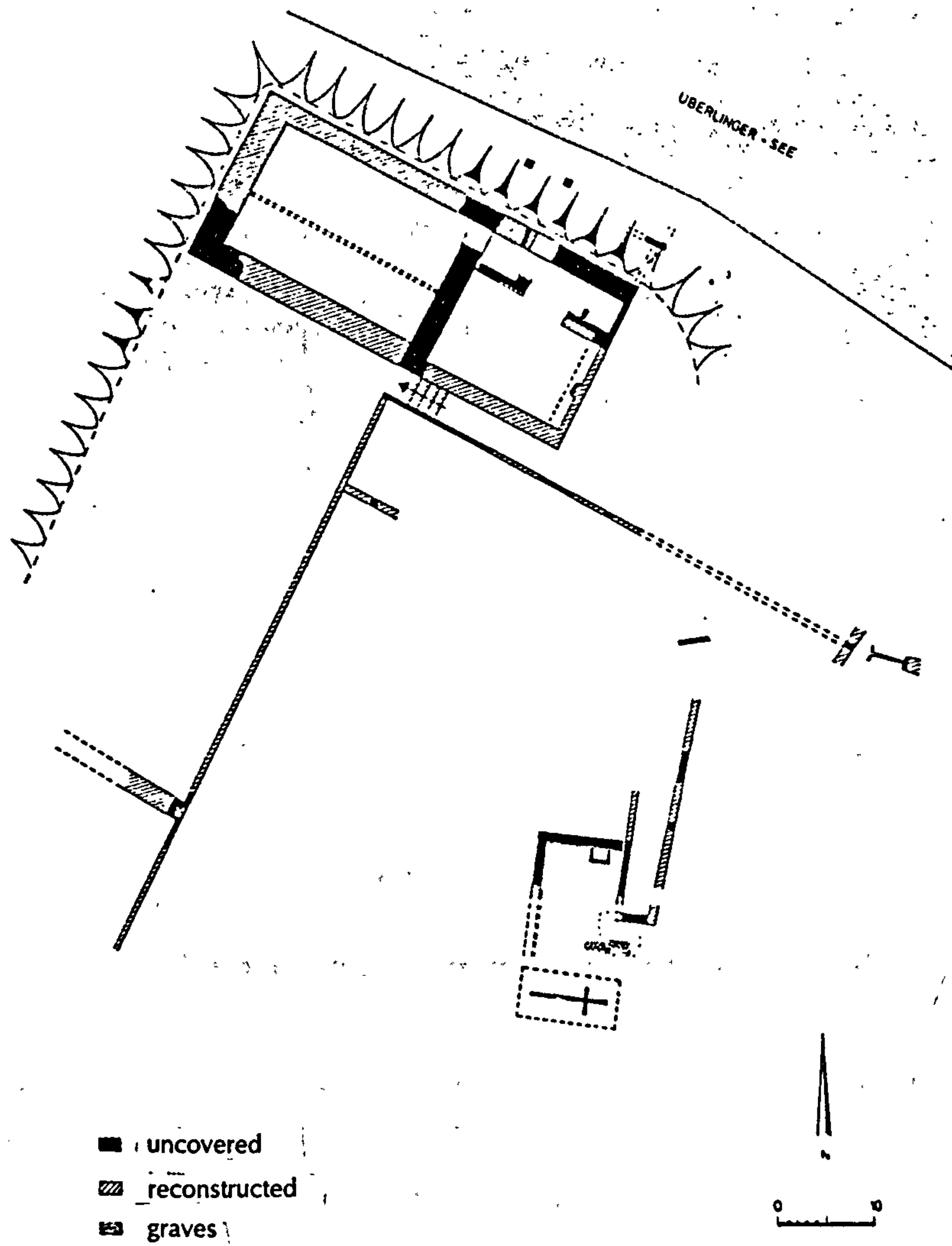


Fig. 6.14 Bodman Carolingian royal hall (after Erdmann).

known. The enclosure wall around the church was not a substantial one, so that the excavator refuses to consider it defensive. The enclosure wall passed so close to the hall that it left only a small passage between the two. This passage appears to have been stepped, perhaps leading ultimately down to the shore. Continuing through this passage one finds a 'courtyard' of sorts just south of the hall, formed from the steep natural fall of the land on one side, the hall and churchyard enclosure on two others, and a large barrier wall on the final side.

Bibliography: Erdmann 1979.

Zürich (Switzerland). A building, 14 x 40 (at least) metres, has been discovered at the Lindenhof, overlying an earlier Roman *castellum* and presumed Merovingian constructions, on the bank of the Limmat by the Zürichsee (fig. 6.15). Two rooms were separated from the main hall at the northern end and a

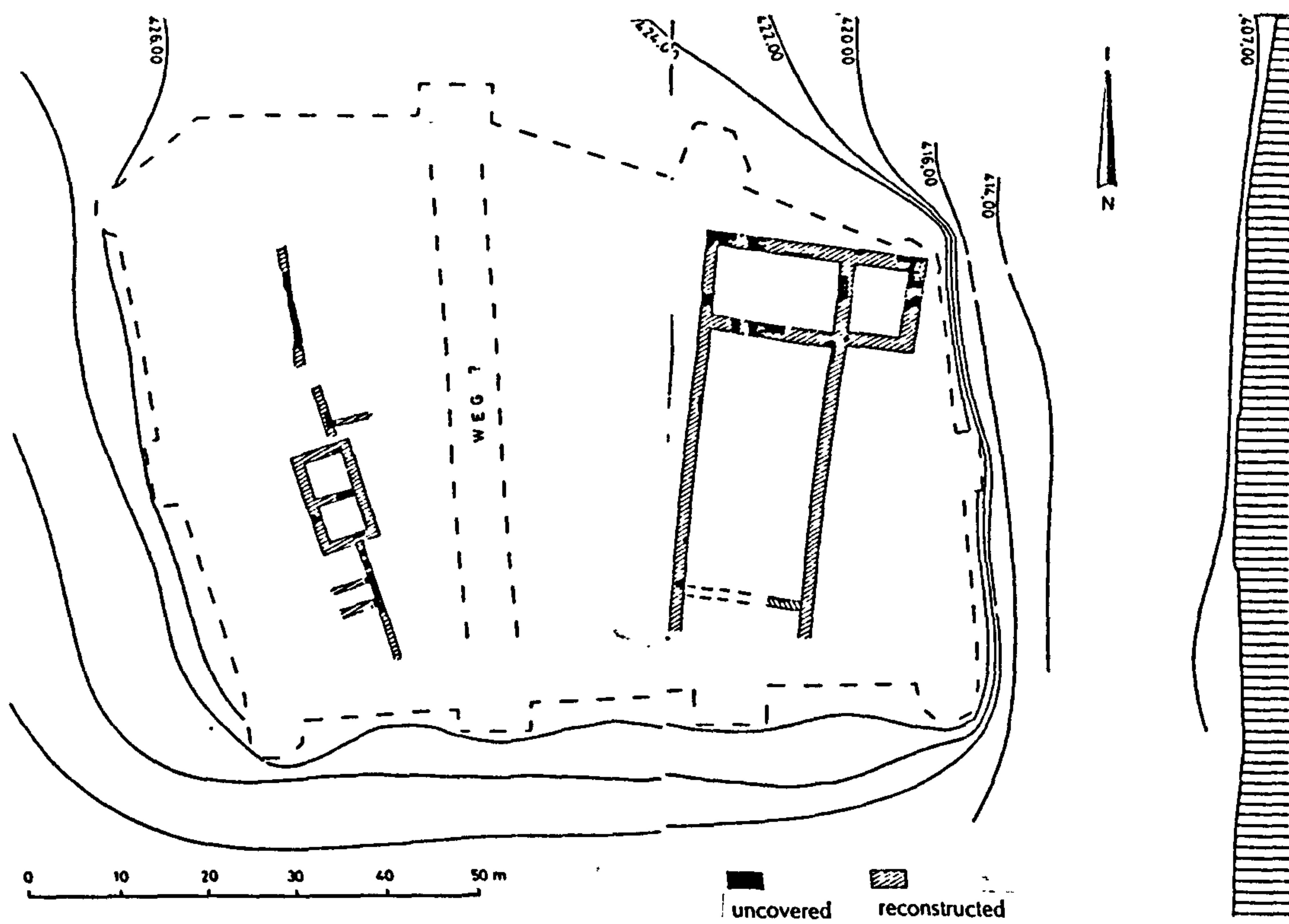


Fig. 6.15 Zürich Carolingian royal hall (after Vogt).

further room may have lain south of the main hall, but the entire length could not be ascertained. The foundations of this mortared-stone building were 1 m. wide, thus leading to the interpretation that it was at least two-storeyed. It is possible that the small eastern projecting room was the base of a low tower, for its foundations were slightly wider than those of the rest of the building. The excavator claims that the construction techniques were not of the highest order as the mortar was poor and the room corners nowhere formed exact right angles.

Further to the west was a building of much less impressive nature which has led to the interpretation that this was a 'work' area and that the large building to the east was the dwelling or some monumental construction. It was in a prominent position, overlooking the Limmat, and from there one could have been seen a long distance. The Roman towers and walls certainly no longer stood, and no enclosure replaced them. The site, like Bodman, stood on a little hillock of difficult access, made more difficult by having been raised on the platform of Roman rubble.

Dating evidence was largely missing. A section of what was probably a pilaster capital is thought to be best dated to the ninth century, although this has been considered too narrow. A definite *terminus ante quem* is given by the Ottonian palace which overlay it. This later palace, regularly visited by kings,

appears in tenth-century documents. It seems acceptable to assume a Carolingian predecessor (we know a *curtis regia* existed in Zürich in 873) and the phase in fig. 6.15 is generally interpreted as being connected with the foundation of the abbey of Fraumunster by Louis the German in 853. If the evidence does not allow such an exact claim, royal ownership and construction in the mid-ninth century must certainly be accepted as likely and thus sufficient for our purposes.

Bibliography: Erdmann 1979, Vogt 1948.

Zullenstein bei Nordheim (Biblis, Kr. Bergstrasse, Hessen). At the confluence of the Weschnitz and the Rhine lie the remains of the medieval castle Stein. It lies directly over a late Roman *burgus*, a small military outpost tower, 16.1 x 22.3 m. with 2.6 m. thick walls, surrounded by a ditch and short rampart which all protected a boat landing. It was built in the last quarter of the fourth century as part of Valentinian's strengthening of the *limes*.

In the Carolingian period the tower was reinhabited, with a dividing wall inserted making two narrow rooms, 2.5 and 2 metres wide respectively (fig. 6.16). A series of further rooms of small limestone blocks, the walls of which were only some .9 m. wide, were built on the south side over the late Roman enclosure wall and tiny corner turret, which were pulled down, presumably, to the floor level of the new building(s). The southern construction consisted of two rooms adjoining the *burgus* tower (although the excavator remains uncertain as to whether these remained in use for long) and a large rectangular room with a further 4.8 x 3.7 m. rectangular addition at the end of which was an apse. The so-called hall and chapel were clearly tied together in a single act of construction. Fragments of an altar were found within the smaller rectangular room and there is no doubt that this formed part of a chapel. The question that does arise is whether the entirety served as a church or whether, as the excavator believes, the larger room served as a hall with a small chapel off its eastern end. A cemetery, consisting of W-E oriented graves completely without grave-goods, lay just east of the building complex.

Further timbered buildings were found to the north of the site, although badly disturbed for a medieval tower was later placed within the *burgus* tower, the remaining walls of which must have served as a tiny enclosure. Around the whole site was dug a tremendous ditch and earthen rampart (the former destroying much of the evidence of further Carolingian occupation).

The finds from the site, almost exclusively pottery, derived mainly from disturbed contexts and date from the second half of the eighth and ninth

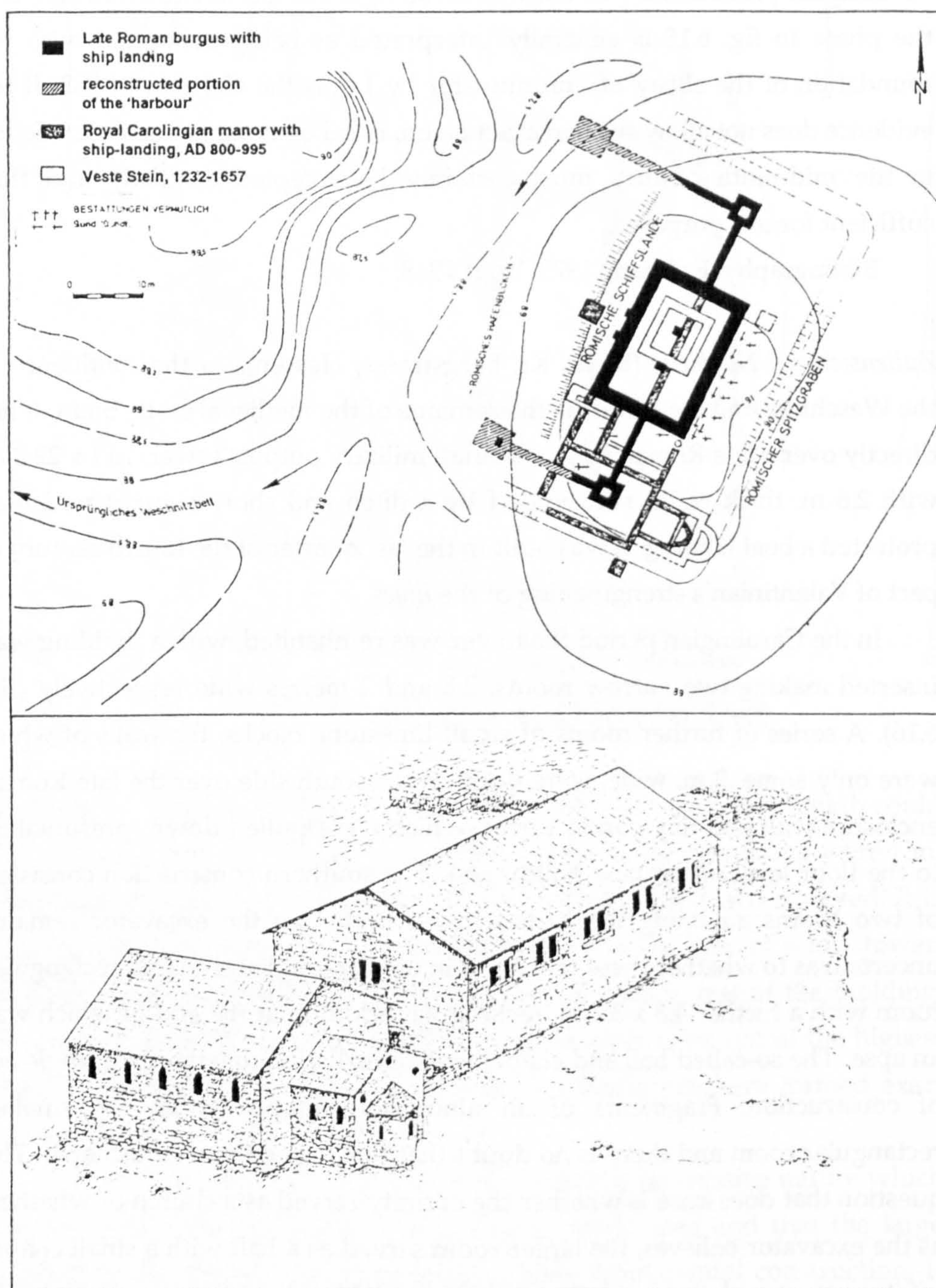


Fig. 6.16 Zullenstein Carolingian royal buildings on top of the remains of a late Roman burgus (after Jorns), and reconstruction (after O. Behrens).

century. The eighth-century finds derived from east of the stone buildings, which themselves yield finds from the very end of the eighth and from the ninth century.

Documentary evidence reveals that Zullenstein was royal. It appears in a series of donation to Lorsch abbey in 806, 836, and 846, in which charters it was

referred to as a *villa* or *curtis*. The reference to a *portus* in the 846 charter has captured most scholars' interest. It has been suggested that when Einhard left Strasburg in 827 with relics of the saints Marcellinus and Peter, en route for his church at Steinbach, that he stopped, (*ad locum qui portus vocatur*) at Zullenstein. A royal villa would make a reasonable overnight stopping point. The only archaeologically discovered evidence of the port function was the large foundation block directly in front of the villa entrance, lying directly on the former river bank. It is thought to have been the base for a wooden crane to help unload ships. Documentary evidence reveals that Zullenstein was granted a market in 995, although archaeologically nothing of the Ottonian period has been found. (all refs. to documents in Jorns 1979, 113, 121-122).

Bibliography: Jorns 1973, 1979, Knöpp 1974.

Samoussy (Aisne). Samoussy lies in flat country just east of Laon. The site of the palace, in the centre of the modern village, is on a slight rise surrounded by low-lying, marshy ground. The fact that Samoussy is only a tiny village and has never been densely occupied since the Middle Ages means that the remains of the Carolingian palace (or at least its foundations) are fairly well preserved. Excavation of Samoussy was undertaken by Georg Weise during German occupation of the area in the First World War. Although this means the technique was one of following walls once uncovered, the execution of the technique was, at least, careful and systematic.

There was clearly no Roman precursor at Samoussy; no Roman finds were turned up. Contemporaneity of walling was established by the characteristic use of a hard yellow quarried limestone bound with a crumbly yellow-brown mortar. A *terminus ante quem* is established by the early Gothic church, under which walls run. But that the buildings date to the Carolingian period can scarcely be doubted. Samoussy is first mentioned in 768 when Pippin celebrated Christmas there. Carloman died there in 771 and it was visited by Charlemagne, Louis the Pious, and Charles the Bald, who all attested charters there. It was at Samoussy that Charles the Bald received a letter from pope Nicholas I. By the early twelfth century, however, Samoussy had sunk into obscurity. There was no secular lord of any pretension and most of the property was in Church hands. There seems no date outside of 750-900 that could be attributed to such monumental buildings.

At the north-eastern limit of the archaeological discoveries, built within the village gateway, was what may have been the original palace gateway. Weise thought the gateway might have been flanked by towers (the complete

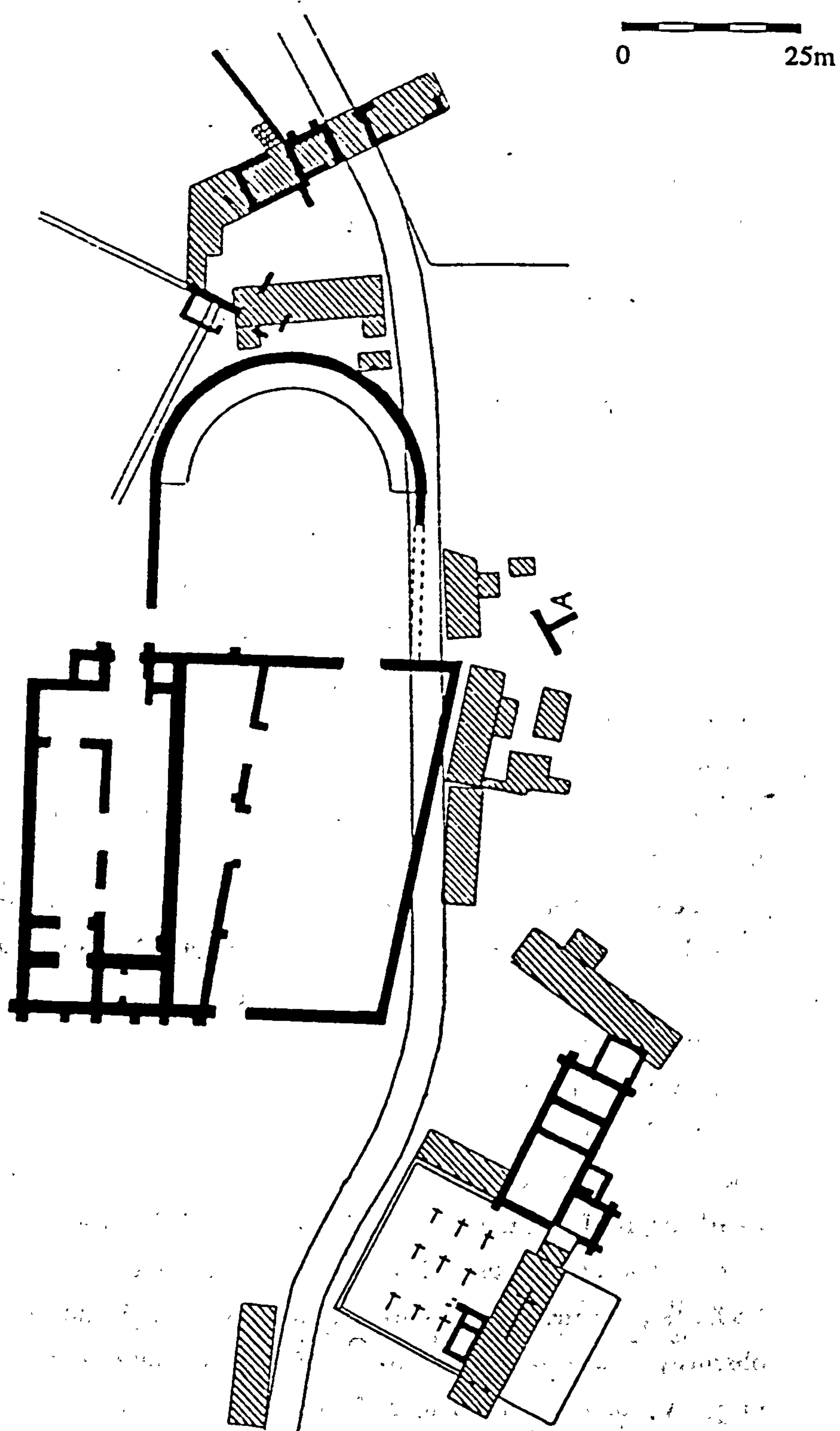


Fig. 6.17 Samoussy Carolingian palace (after Weise).

foundations on one small square room was found). A stretch of otherwise inexplicable wall may have been an enclosure wall, which acted as a funnel into the gateway, but no other evidence for an enclosure wall was found. A building directly next to this gateway opens outwards directly behind this possible enclosure wall, in front of which was well-built stone flagging.

The Hall. The extremely large, 50 x 25 m., rectangular hall was subdivided into a range of rooms along the north side and one or two rooms at the western end. The long walls were 2 m. thick and suggest that the building was two storeyed. The excavator suggested that the ground floor rooms were for storage or work and that the main hall was on the first floor. In the south-east corner the traces of what might have been the first step of a stairway was discovered. At the eastern entrance to the hall were two small 5m. square rooms that may have been towers flanking the doorway.

To the south of the hall was a quadrilateral enclosure, subdivided near the hall. To the east of the hall, opening up just by the entrance to the hall was a remarkable semi-circular enclosure, consisting of a single wall 2.25m. thick. Weise was insistent that no stone walls abutted either of these enclosure walls, which must have been free standing. Much too wide to have been roofed, they must be seen as courtyards, unless there were wooden lean-tos, but this seems unlikely. The interior of the semi-circular enclosure wall was faced with stone slabs of 30-60 cm. in length and 15 cm. thick, where the wall stood above the foundations. Weise liked to see here an ornamental garden.

The Church. Excavation inside the early Gothic church was not possible. A Carolingian church may underlie it. Two small barrel-vaulted cellars protrude from under the church to the north. Weise discounted the possibility of them being crypts, but for rather unconvincing reasons.

To the east of the church and similarly oriented is a building, some 20 x 8 m., subdivided into three rooms. In plan it is hard to accept it as a church itself. And while it would appear to be connected with the church somehow, the Carolingian precursor would have been much smaller than it to fit underneath the Gothic church.

Bibliography: Weise 1923

Doué-la-Fontaine (Maine-et-Loire). The possibility of a Roman predecessor is raised by an amphitheatre some distance from Doué and the suggestion that Doué represents the site of Segora from the Reutinger table. But such a possibility remains speculation.

The memory of a Carolingian palace is perhaps to be found in the church dedication of Notre-Dame de la Chapelle. Two mottes are located in Doué, one of which is sited in the area known as la Chapelle. Encased within it was an earlier building standing 5 to 6 metres high. The walls of this rectangular 16.5 x 23.5 m. building were just under 3.5 metres thick. Foundations varied from being non-existent to being dug nearly a metre deep. The walls were composed of two types of stone: one of rectangular shape with sides varying from under 20 to just over 30 cm, the other quite flat and thin. These flat stones were laid in herring-bone fashion. Mortar was used copiously and in a very liquid state. The eastern and western walls stand more or less to their full height while evidence exists to show that the northern and southern walls were gable ends, although how much higher they continued and thus the pitch of the roof is unknown. No windows were preserved in the eastern and western walls, although it is possible that windows were lost in later alterations. On the northern and southern side, single windows were located at heights over 5 metres from the ground. Doors were found in the southern and western sides. An internal dividing wall was added with a chimney built of brick set into the southern side. The smaller room thus formed has been interpreted as a kitchen, distinguishing itself from the larger northern room by not having a paved floor but one of beaten earth. Neither had it been kept clean. Another addition was the southern annex, in which was found a deep pit.

The whole structure was very seriously burnt at some stage, reddening the interior walls and sealing the area with a thick layer of burnt material. A C14-date from this charcoal was 750±100 (uncalibrated). The burnt layer also sealed a coin dating to the last decade of the ninth century. At some later date, suggested as 950-960 on coin evidence, reconstruction involved sealing up doors and windows and heightening the building, thus forming a donjon.

The excavator refuses to believe that any trace of the Carolingian palace remains (de Bouard 1973-4, 9). The building described above is dated by him to ca. 900 AD. The C14-date does not throw such a date into doubt, but it better supports a Carolingian date. Moreover, the coin dates the conflagration to the first half of the tenth century. A date anywhere in the ninth century must be allowed for the building's construction as probable, although the attempt to equate the building with Louis the Pious should be resisted. That it was a ninth-century royal building (probably the major building of the palace) that passed into the counts' hands seems highly likely.

Bibliography: de Bouard 1973-4.

Doué completes the record of those royal villas for which there is information derived from excavation, and at best other sites can only offer us plausible information about where the villa is probably to be found. It will suffice to give just one example.

Herstal (Liege, Belgium). Herstal lies on the left bank of the Meuse only 6 km. north of Liege and through it passed the Liege-Maastricht Roman road. From the west, the Roman road from Tongres crossed the Meuse just south of Herstal, heading for Jupille. Along the north-south road to Maastricht lay a number of Roman villas. One such site, apparently abandoned in the late third century, lies to the north of Herstal. Another possible villa lies to the south. Here a cemetery has been located with fourth- and fifth-century burials. Whether there was continuity of the site into the Carolingian period is impossible to say. In 716 the villa Cheristalius was mentioned as a stopping place for the cortege of Saint Lambert. A miracle prompted the building of an oratory in the following years, almost certainly at the spot of Saint-Lambert chapel. In 723 Charles Martel issued a charter from the villa Harastallio which is first called a palatium in a diploma of Pepin the Short in 752.

The site of the palace is thought to be at Licour, largely because of the presence of the parish church dedicated to Notre-Dame. The fifteenth century 'tour de Pépin' hardly points to unbroken continuity, for already in the thirteenth century legends connecting Pepin with Herstal were being written down. Should the site prove indeed to be the palace site, it lies on a very slight eminence, on both sides of which flow fine streams into the Meuse.

Bibliography: Joris 1973.

In addition to the above archaeological examples, we have an important document which must be considered; the relevant sections of the *Brevium Exempla* are given here before the next chapter summarises what we know of royal Carolingian villa architecture (translation from Loyn and Percival 1975).

Brevium Exempla ad describendas res ecclesiasticas et fiscales.

[The first nine paragraphs concern the church of St. Michael on an island called Staffelsee, its furnishings and its dependent estates. Seven *curtes* in the diocese are mentioned as not being recorded. One is described with all its attendant arable and meadow, livestock, foodstuffs, household items, and agricultural tools. Dependent on this *curtis* were 23 free and 19 servile manses, their renders in kind and service being similarly recorded.]

7. As above. We found in the same place (Staffelsee island) associated with the church already mentioned above, a *curtis* and demesne house with other buildings . . . (*curtem et casam dominicatam cum ceteris aedificiis*).

Concerning those clerics and laymen who have given their properties to the monastery called Wissembourg, and in return have received the usufruct of them.

10. Hartwic the priest has given to the monastery mentioned above, in the *pagus* of Worms, half of the church which is constructed in the villa Hessiheim, and with the demesne house, 4 servile manses – all occupied – (*in villa Hessiheim, et cum casa dominicata mansos vestitos serviles iv*), 5 *picturae* of vineyard and meadow yielding 20 loads of hay. In return he has received the church in the villa Unkenstein and with the demesne house, 6 servile manses – all occupied – (*in villa Unkenstein, et cum casa dominicata mansos vestitos serviles vi*), on condition that he shall hold what he gave in precarial tenure for the rest of his life.

11. Motwinus and his wife have likewise given to the monastery in the villa Hessiheim in that *pagus*, with a demesne house, 6 servile manses – all occupied – (*in villa Hessiheim . . . cum casa domincata mansos vestitos serviles vi*), 5 *picturae* of vineyard and meadows yielding 12 loads of hay. In return they have received a demesne house in the same villa with 6 servile manses – all occupied – (*in ipsa villa cum casa dominicata mansos vestitos serviles vi*), 7 *picturae* of vineyard, and meadows yielding 15 loads of hay.

12. Unroh has likewise, in precarial tenure for the rest of his life, in this *pagus*, in this villa, 1 occupied manse, 2 servile manses which are vacant (*in villa illa mansum vestitum i, serviles absos ii*), 1 *pictura* of vineyard and meadows yielding 20 loads of hay.

[In the next twelve paragraphs *casae dominicatae*, servile manses and even two mills are to be found held in precarial tenure or as a benefice in nine other named *villae* in the *pagus* of Worms.]

Concerning the district of this or that mayor.

25. We found on the royal estate of Asnapius a royal house, well built of stone, with 3 rooms; the whole house surrounded by galleries, with 11 rooms for women; underneath 1 cellar, 2 porches; 17 other houses inside the courtyard, build of wood, with as many rooms and with the other amenities all in good order; 1 stable, 1 kitchen, 1 bakehouse, 2 barns, 3 haylofts. A courtyard with a strong *tunimo* and a stone gateway with a gallery above from which to make distributions. A smaller courtyard similarly enclosed with a *tunimo*, well ordered and planted with various kinds of trees.

(*Invenimus in Asnapio fisco domico salam regalem ex lapide factam optime, cameras iii; solariis totam casam circumdatam, cum pisilibus xi; infra cellarium i; porticus ii, alias casas infra curtem ex ligno factas xvii cum totidem cameris et ceteris appendiciis bene compositis; stabulum i, coquinam i, pistrinum i, spicaria ii, scuras iii. Curtem tunimo strenue munitam, cum porta lapidea, et desuper solarium ad dispensandum. Curticulam similiter tunimo interclausam, ordinabiliter dispositam, diversique generis plantatam arborum.*)

[There then follows a description of the household items, food supplies and livestock.]

26. Concerning the same as above. Concerning the manses (*mansionilibus*) which pertain to the above mentioned demesne (*mansum*). In Grinsione villa we found demesne manses with 3 haylofts and a courtyard enclosed with a *sepe* (*mansioniles dominicatas, ubi habet scuras iii et curtem sepe circumdatam*). There is a garden with trees, 10 geese, 8 ducks, and 30 chickens.

27. In another villa we found demesne manses with a courtyard enclosed with a *sepe* and inside (*In alia villa repperimus mansioniles domincatas et curtem sepe munitam, et infra*): 1 *aripennis* of vineyard, a garden with trees, 15 geese and 20 chickens.

28. In the villa -----, demesne manses. It has 2 haylofts, 1 barn, 1 garden, a

curtis well enclosed by a *sepe*. (*In villa illa mansioniles dominicatas. Habet scuras ii, spicarium i, ortum i, curtem sepe bene munitam.*)

[The next paragraph describes the foodstuffs and plants found there.]

30. We found in the fisc -----, a royal house, well built of stone outside and wood inside, with 2 rooms and 2 galleries; 8 other houses in the *curtis*, built of wood; a woman's workshop with 1 room built ordinarily; 1 stable; a kitchen and bakehouse combined; 5 barns; 3 granaries. A *curtis* enclosed with a *tunimo* with spikes on top, with a wooden gateway. The gateway has a gallery above it. A smaller *curticula* similarly enclosed with a *tunimo*. A contiguous orchard . . . (*Repperimus in illo fisco dominico domum regalem, exterius ex lapide et interius ex ligno bene constructam; cameras ii, solaria ii. Alias casas, infra curtem ex ligno factas viii; pisile cum camera i, ordinabiliter constructum; stabulum i. Coquina et pistrinum in unum tenentur. Spicaria quinque, franecas iii. Curtem tunimo circumdatam desuperque spinis munitam cum porta lignea. Habet desuper solarium. Curticulam similiter tunimo interclusam. Pomerium contiguum . . .*).

[Household items, foodstuffs, and livestock described in remaining and next paragraph.]

32. We found on the royal fisc -----, a royal house with 2 rooms and 2 fireplaces, 1 cellar, 2 porches, a *curticula* strongly enclosed with a *tunimo*; inside it 2 rooms with 2 women's houses; a chapel well built of stone; within the *curtis* 2 other wooden houses, 4 granaries, 2 barns, 1 stable, 1 kitchen, 1 bakehouse; a *curtis* enclosed by a *sepe* with a wooden gateway and above it a gallery. (*Invenimus in illo fisco dominico casam regalem cum cameris ii totidemque caminatis, cellarium i, porticus ii, curticulam interclusam cum tunimo strenue munitam; infra cameras ii, cum totidem pisilibus, mansiones feminarum iii; capellam ex lapide bene constructam; alias intra curtem casas ligneas ii, spicaria iv; horrea ii, stabulum i, coquinam i, pistrinum i, curtem sepe munitam cum portis ligneis ii et desuper solaria.*)

[Household items, foodstuffs, and livestock described in remaining and next paragraph.]

34. We found on the royal fisc -----, a royal house, ordinarily built of wood with 1 room, 1 cellar, 1 stable, 3 houses, 2 barns, 1 kitchen, 1 bakehouse, 3 haylofts. A *curtis* enclosed by a *tunimo* and above it a *sepis*. A garden planted with trees of various kinds. 2 wooden gateways. (*Repperimus in illo fisco dominico domum regalem ex ligno ordinabiliter constructam, cemeram i, cellarium i, stabulum i, mansiones iii, spicaria ii, coquinam i, pistrinum i, scuras iii. Curtem tunimo circumdatam et desuper sepe munita. Ortum diversi generis insertum arboribus. Portas ligneas ii.*)

[Household items, foodstuffs, and livestock described in remaining and next paragraph.]

36. We found on the royal estate Treola, a demesne house, very well built of stone; 2 rooms with 2 fireplaces, 1 porch, 1 cellar, 1 wine-press, 3 houses for men built of wood, 1 barn, 1 gallery with a women's house; 3 other buildings of stone, 1 barn, 2 haylofts, a *curtis* enclosed by a wall with a gateway built of stone. (*Invenimus in Treola fisco dominico casam dominicatum ex lapide optime factam, camaras ii cum totidem caminatis, porticum i, cellarium i, torcolarium i, mansiones virorum ex ligno factas iii, solarium cum pisile i; alia tecta ex maceria iii, spicarium i, scuras ii, curtem muro circumdatam cum porta ex lapide facta.*)

[Household items, plants and trees described in remaining and next two paragraphs, followed by the instruction to make a summary of all the figures for the various items recorded.]

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Chapter Seven

Carolingian Villas, Palaces, and Estates

There are three problems that conspire to make the study of Carolingian lordly residences in Germany almost impossible. One is that German archaeologists tried to appropriate the medieval term *curtis*, to use it for their own site typologies. The confusion this produced and the berating they received from historians has resulted in the total abandonment of the term by archaeologists. With it has come the illogical belief that nowhere has a *curtis* or *villa* been excavated. Many a probable lordly *curtis* has been excavated and called everything but *curtis*.

The second problem is that, having excavated enclosed and ramparted sites, archaeologists have struggled to know what to make of their murky and very incomplete evidence. There is a widespread belief that the many sites, enclosed by palisade or even mortared stone walls, were never occupied. Many a probable lordly *curtis* has been deemed an empty refuge.

The third problem is that German medieval historians have, for a variety of reasons, been ready to attribute almost everything, every site and every historical development, to the monarchy. Carolingian kings are accredited with having planned, planted, organised, settled, administered, protected, and exploited, every excavated enclosed site and many a village, even some devoid of any evidence for their existence before the eleventh century! Many a probable lordly *curtis* has been deemed a royal marching camp fort.

Before the problems can be addressed, let me summarise the results of excavations at four enclosed Carolingian sites east of the Rhine. These few must suffice, for as von Uslar noted (see chapter five), with the start of the eighth century the amount of archaeological evidence for *Burgen* in Germany multiplies dramatically. Even if I had continued with the minimalist approach adopted in chapter five, covering the eighth and ninth century would have entailed discussing at the very least three dozen sites.



Fig. 7.1 Büraburg bei Fritzlar (1:5,000) (after Schwarz).

Büraburg (Fritzlar, Schwalm-Eder-Kreis, Hessen). The mission of Boniface established 3 bishoprics in Franken. One was at *Büraburg*, which Boniface called an *oppidum* in his correspondence with Pope Zacharias in 741/2. Only one bishop was ever consecrated, Witta, who appears not to have survived more than a handful of years. After his death the area passed into the direct jurisdiction of Boniface himself, who was then bishop of Mainz. The church of St. Brigit, which sounds suspiciously like an earlier dedication from the Irish missions, overlies a church, 24 x 9 m., composed of western tower (4.5 x 4.8 m.), rectangular nave, and narrower rectangular choir. This is certainly the eighth-century church of Witta. The associated burials are almost entirely without grave goods, in keeping with eighth-century burial practice.

Büraburg lies three kilometres from Fritzlar and is easily seen from there, for it sits some hundred metres above the Eder river (fig. 7.1). Although the site falls away towards the Eder on the north, this side does not provide a steep

approach to the site itself, once the mighty hill has been climbed. The eastern and western sides, on the other hand, provide fairly steep approaches. The south side was always the main entrance. Here a saddle separates the site from the rest of the sandstone upland. Here a number of ditches defend this natural approach, although unlike Christenberg these are contemporary with the Carolingian fortification and are not Iron Age.

The oldest wall on the site encloses 8 ha, and is a mortared stone wall 1.5 m. thick (thus about 5 Carolingian feet), although in some places nearly reaching 2 m. in thickness. In nearly its entire length this wall was replaced by a second mortared wall 1.8 m. thick (6 Carolingian feet). In many ways the replacement wall was of better construction, including deeper foundations. This second wall was patched up, occasionally in long stretches, at a subsequent date. The walls were constructed of an inner and outer face of carefully chosen stones and a rubble core with generous amounts of mortar. The outer face was more carefully constructed than the inner, of larger blocks which came from a local source. Limestone from nearby Eckerich was brought to the site for burning to make mortar. The wall, it would seem, was built by teams judging from joins and occasional variances of direction.

Towers existed in each phase, according to Norbert Wand, although the only good example is that of the south-west corner. Here a 6.5 m. square tower served as a gate-tower in the second wall period. In its earliest period Büraburg possessed three gateways. The southern gateway was a minor one and the south-west gate-tower was presumably there only to serve the small settlement that lay outside the walls there. The southern gateway was formed of a simple gap in the walls and led down the hill very steeply to the Eder valley. The northern gateway led instead around the side of the fortification and from there to what now forms the approach road. This entrance consisted of parallel ends of the wall, which created a 30 metre-long passage. Thus the three gateways were of three different types. The walls served Büraburg well, for in 774 the *RFA* record the unsuccessful siege of the site by Saxons.

The buildings within Büraburg are interesting. The best investigation of the interior concentrated on the south-east corner; elsewhere little excavation can be said to have been undertaken. Here a row of timber-built structures were found butting the rampart wall: some 25 altogether, according to the excavator. Along the eastern wall, nine hearths were discovered in timber dwellings measuring 3 x 7.5 m. with shared dividing walls. Between 4 and 5 m. from the rampart wall, a small ditch took doubled posts, perhaps this also served to divide the interior into two sections. A similar series of twelve buildings were

found along the southern wall, the fifth and sixth from the gate-tower apparently yielding evidence of smithing. These houses were built after the construction of the second period enclosure wall (it is this wall the houses abut). A similar series of buildings against the rampart wall were found earlier in the century by Vonderau by the northern gateway, although different in that the wall foundations were marked by a line of stones on which it is thought the wooden walls were built.

How these buildings were roofed is a difficult question, unless we consider a structure like a lean-to, with a continuous roof sloping from the rampart wall forward into the interior. This could then be the meaning of the doubled posts running at a regular line 4-5 metres from the eastern rampart wall face.

Finally, a large free-standing timber building, 7.5 x 7.5 m., with a central fireplace was found by the south-east gate-tower; a *Grubenhaus* was found in the centre of Christenberg; and a strange unexplained construction was uncovered just south of the northern gateway.

The number of buildings has impressed some, for only 8% of the site has been excavated, and caused a great deal of speculation about the meaning of *oppidum* in Boniface's letter to the pope. Much of the argument about whether Büraburg was 'truly' urban is gratuitous, but the site was clearly important, as were the other Bonifacian choices for bishoprics, such as Erfurt and Würzburg. It is hard to know whether Büraburg was not quite in their class, given that its importance was so short lived, but a case could be made for it being of equal rank. It would appear that Boniface's monastic foundation, with him as abbot, at Frizlar sapped Büraburg of its importance. The almost immediate overshadowing of Fritzlar by Fulda must have assured the demise of Büraburg in the same way that the movement of an episcopal seat in the fifth century caused the demise of several Gallic cities (chapter two).

Bibliography: Gensen 1975b, Wand 1974, 1975, and in Roth and Wamers 1985, Vonderau 1934.

Kesterburg auf dem Christenberg (Münchhausen, Kr. Marburg-Beidenkopf, Hessen). Twenty kilometres north of Marburg, Christenberg lies on the eastern edge of the Burgwald, a forested region covering infertile sandstone hills. The site is only first documented in 1225 as Kesterburg. *Kester* is taken to be derived from *castra*, and Frankish. It might be noted, however, that the Anglo-Saxons adopted *caester* among only a few other Latin loan words, and it is not inconceivable that the indigenes of Hessen similarly borrowed the word, so that the name *Kester* hardly need be taken as proof that Franks gave the name to

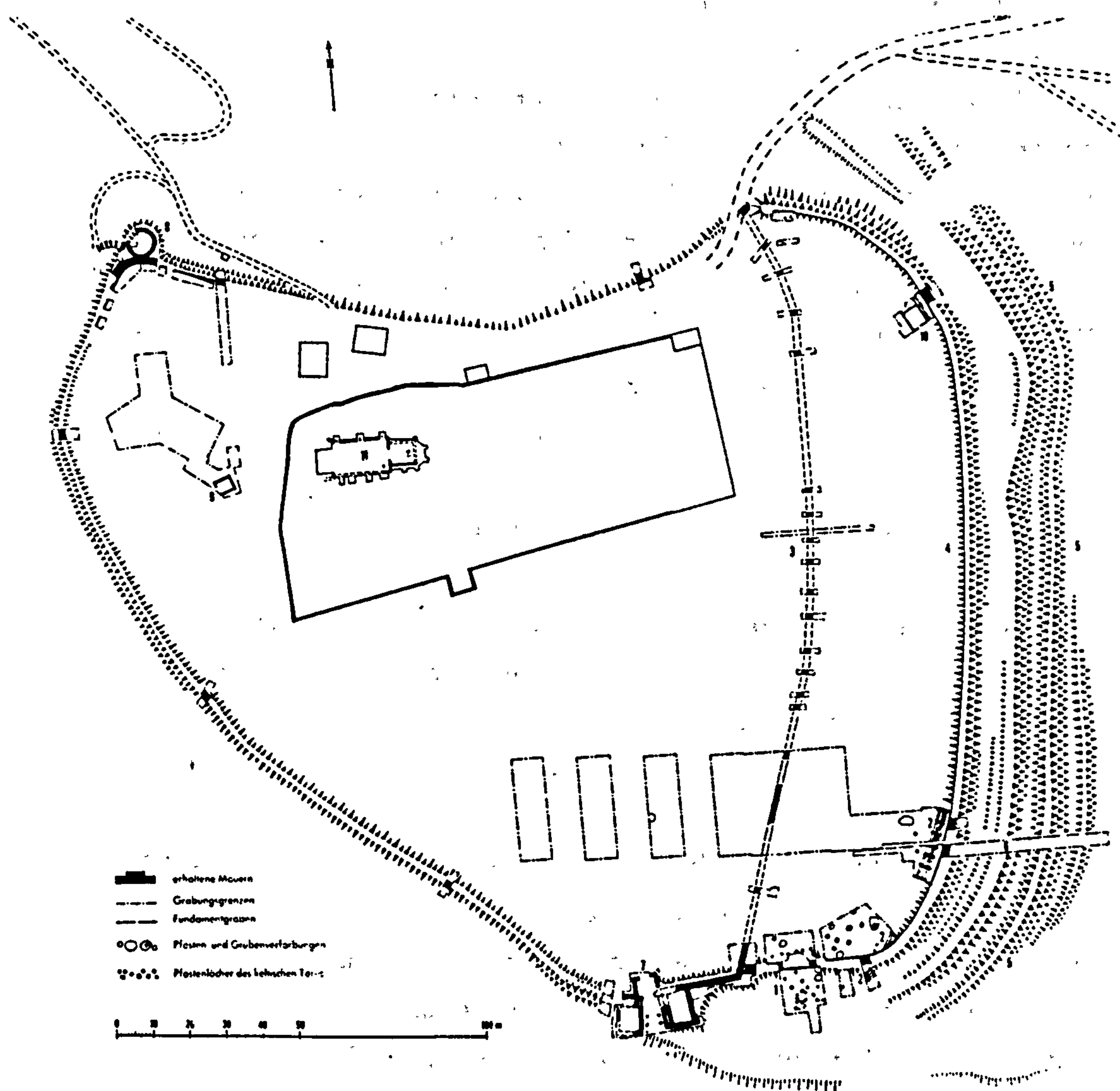


Fig. 7.2 Christenberg (1:5,000) (after Gensen).

the site, far less that they built it. It has been further suggested that the site gave its name to the Burgwald which would later be recorded as being a royal forest. Taken together, it has often been proposed that the site was established by Frankish royalty. At a height of 400 metres, the Christenberg lies only two kilometres from the Weinstraße, an ancient route running from Frankfurt north towards Paderborn. The importance of this route has also been suggested as the reason for royal interest in a fortification here.

The early medieval fortification was built on top of the remains of a La Tène hillfort dated to 447 BC (fig. 7.2). The first phase of the wall, 1.8 m. wide and mortared, ran along the Iron Age rampart, except along the eastern side. Here it ran through the plateau reducing the just under 4 ha. La Tène enclosure to 3 ha. The wall had no external ditch and no foundation and it probably was not long in use before a new wall, 2.2 m. wide, replaced it and was extended in

the east to run along the innermost of the seven Iron Age ramparts. This wall revealed at least three phases of reconstruction.

The south gate was the main entrance for the road leading to the west. The entrance was composed of a 6 x 7 m. gate-tower with a 2.8 m. wide entrance, existing from the first period and once renewed, and to which two rectangular outer bastions were added in a later period. The two bastions thus created an extension to the entrance passage and between them was set a wooden gate. Within the gate-tower and the eastern bastion were found painted plaster of red, green, yellow, and dark grey of seemingly geometric designs. Fragments were found even among the uppermost rubble layers and suggest that upper storeys also had painted plaster walls. Despite stone robbing the vast amounts of rubble suggested to the excavator that the gate-tower stood three stories and the bastions at least two stories tall.

The north gate was created in the second period, the original wall having had no entrance here. It similarly composed a gate-tower, preceded by a wooden gate entrance and a single bastion.

In the north-western corner a round tower, 8.5 m. in diameter, was added in the third period. Less than 10 m. to the east of the tower, just in front of the northern wall was a well, which the tower was doubtless intended to protect.

The church on the Christenberg, dedicated to St. Martin, is today composed of an eleventh-century tower and nave with Gothic choir. Excavation and renovation revealed a 7 x 7 m. choir and 9.7 x 16.5 m. nave which largely underlies the present nave. Although not datable beyond the *terminus ante quem* of the eleventh century, the plan closely resembles that of the eighth-century church of St. Brigit at Büraburg. A slightly later date might be suggested in view of the fact that the St. Martin's church is slightly larger than St. Brigit's which served as an episcopal church in the eighth century.

Few buildings were recognised within the enclosure. A small stone building, 4 x 3 m., with an added room, 4.6 x 1.2 m., was found in the west. Internal post holes and weights were interpreted as evidence of a loom. A timber post building, 5 x 3 m., was found just south of this building with traces of a hearth within. In the eastern section many post holes, pits, and even sections of stone walling were found without any real plans being obvious. Twenty metres outside the north gate, an 11 x 5.5 m. drystone house was found, which was internally divided into two rooms and contained evidence of a wooden roof that had collapsed. Other *Grubenhäuser* were found along the eastern and southern rampart wall and a well along the eastern wall, dated by dendrochronology to AD 810.

Finds from the site date from just before 700 to the first half of the ninth century and were particularly numerous: at least 3,000 pot sherds. The finds would suggest a dense occupation and continuous ploughing is thought to be the cause for the removal of, in particular, archaeological traces of wooden buildings from the interior.

Bibliography: Gensen 1968, 1975a, 1975b, and in Roth and Wamers 1985.

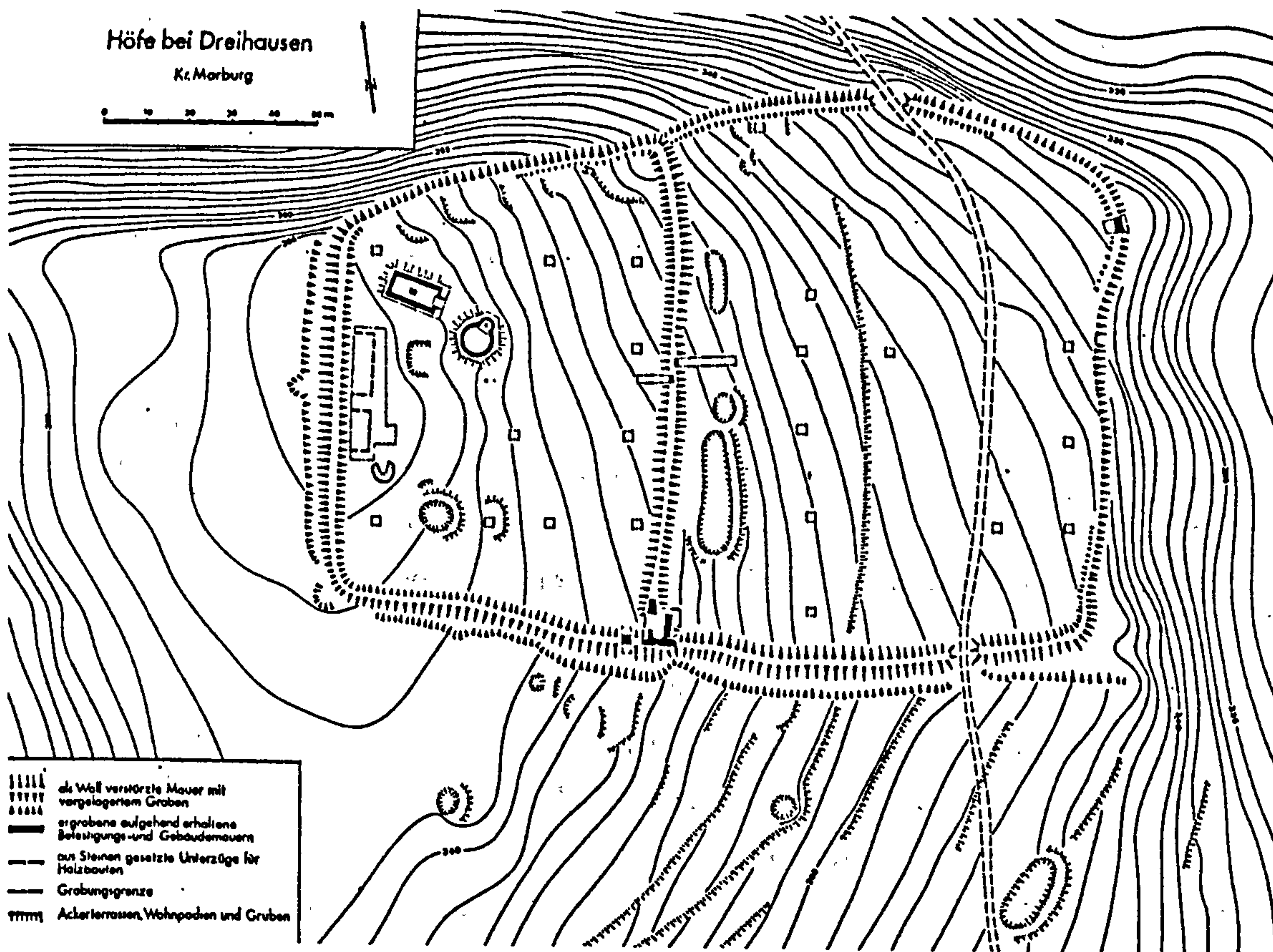


Fig. 7.3 Höfe bei Dreihäusen (1:2,000) (after Gensen).

Höfe bei Dreihäusen (Ebsdorfergrund, Kr. Marburg-Biedenkopf, Hessen). In the south-west of the Amöneburg basin, the Ebsdorfer Grund forms a natural settlement enclave. This is delimited in the south by an east-west ridge, on the northern edge of which can be found the Höfe bei Dreihäusen, overlooking the Ebsdorfer Grund. Kings Henry the Third and Fourth each stayed in Ebsdorf, presupposing a royal estate there in the eleventh century and Charlemagne donated the nearby villa of Rossdorf to Fulda in 781. Together both are taken as possible proof that the site Höfe was also in the hands of the Carolingian kings. It should be noted, however, that no less than four private donors of property in Ebsdorf are documented in the eighth century and one of these owned property of his own in Rossdorf. It would not be surprising if the villa of Rossdorf 'donated' by Charlemagne was in fact the private property of Hartrad

who made it over to the king first, in order to offer Fulda greater protection over its acquisition, as many other 'donations' were so made.

The Höfe consists of two roughly rectangular enclosures, the upper enclosing .75 ha., the lower 1.25 ha., connected by a gateway at the southern extremity of the separating wall (fig. 7.3). The enclosing wall seems to have had only meagre foundations and consisted of drystone, locally available basalt outer faces with a filling of rubble and mortar, with a width varying from 1.25 m. to almost 2 m. wide. A ditch protected the vulnerable west and south sides.

Some two dozen test pits, 2 x 2 m., scattered throughout the interior failed to find any trace of construction in the lower bailey and pottery scatters were much less dense here than in the upper enclosure. The upper enclosure revealed several terraces along the northern wall, and a test pit from one terrace revealed it to be a dwelling. Along the western wall two rectangular ground plans of stone were found and interpreted as stone foundations of wooden buildings, 8.5 m. and 14 m. long. The width could not be determined as the side parallel and nearest the enclosure wall was not found, but if the buildings abutted the wall in would have made them 7 m. wide. A stone-built dwelling, 9.9 x 4.8 m., was found just north of these probably wooden buildings. It was partially subterranean to make a level foundation and contained a central block of stone and mortar, probably a support for a central column. The floor and walls were plastered and traces of burning were found although it was not suggested that this was evidence of a hearth. It was further assumed that there was another storey to the dwelling. Finally and most distinctive of the site was the remains of a circular church, 5 m. in diameter, with wall a metre thick. A small apse existed to the north-east, with a slightly raised floor and the remains of an altar block. The floor and walls of the church were plastered and in the apse at least, there were paintings in blue, black, red, pink, light green, and yellow-brown. Figures 20 cm. high are recognisable, of which at least one was an angel. A fragment of polished green porphyry of Mediterranean origin probably derived from the altar.

Finds belong to the eighth and ninth century, although only a few finds date to the first half of the eighth century. Comparison of pottery with that from Christenberg shows that its origin dates to a few decades later. Occupation does not seem to extend beyond the ninth century and if the round church dates to the earliest part of the occupation period, it would make it the oldest round church north of the Alps. The discovery of some quite exceptional types of pottery further distinguishes the site.

Bibliography: Gensen 1975a, 1975b, and in Roth and Wamers 1985.

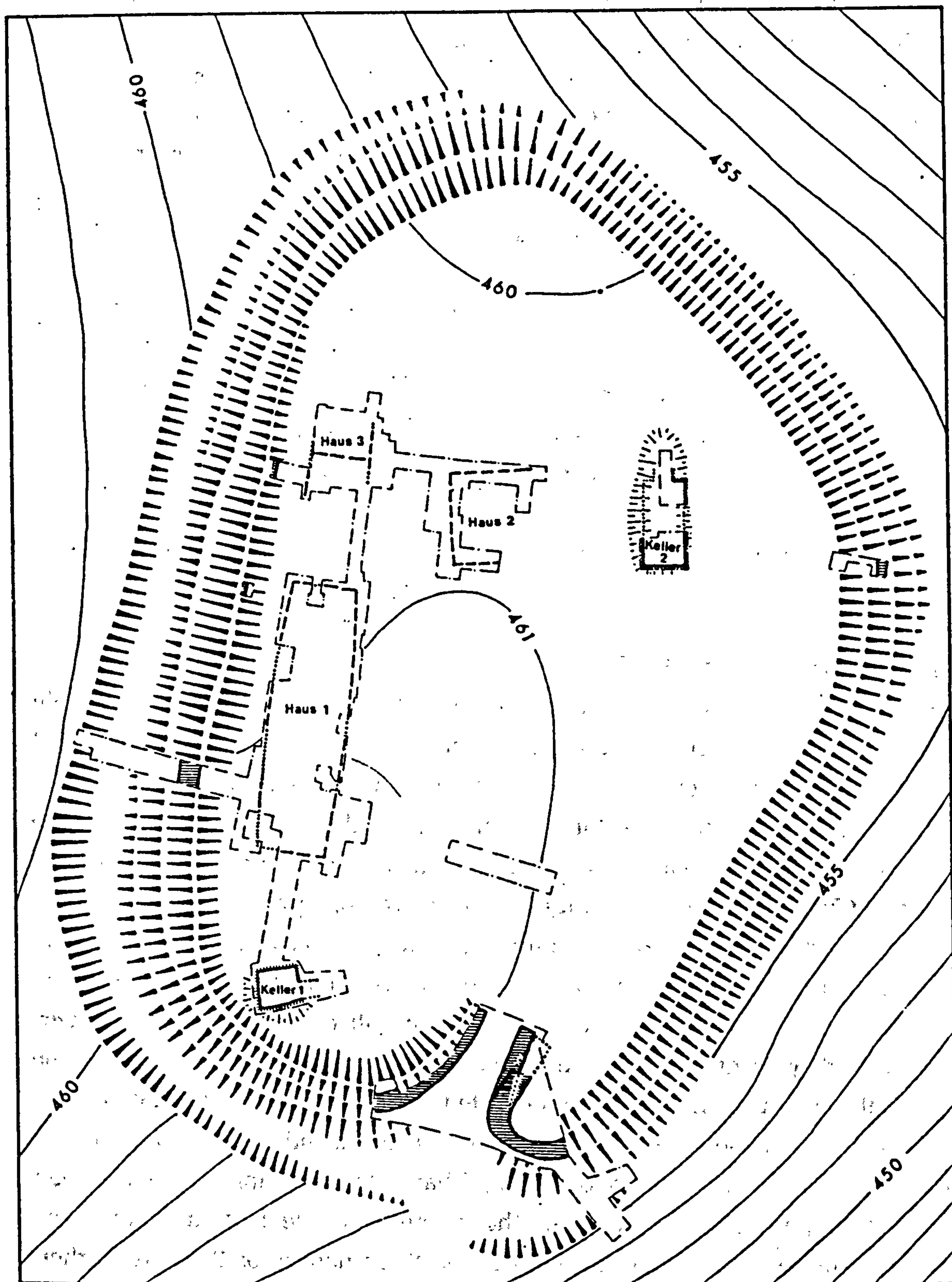


Fig. 7.4 Hünenkeller bei Lengefeld (1:500) (after Gensen).

Hünenkeller bei Lengefeld (Korbach, Kr. Waldeck-Frankenberg, Hessen). Hünenkeller lies near the western edge of the limestone, tree-covered Korbacher plateau, overlooking Lengefeld at the bottom of the steep edge of the plateau and a wide fertile basin to the east.

Here a small enclosure, 75 x 50 m. enclosing only .28 ha., consists of a V-shaped ditch and rampart, which still stands to 3 m. in places, 1.8 m. thick, of stone and mortar (fig. 7.4). Within the enclosure buildings were found in almost every trench opened. A long boat-shaped hall, 23 x 5.7-6.7 m., was the most impressive. Two other houses were partially uncovered, one of which as of similar width to the long hall, although no idea could be gained of its overall length. Two rock-cut cellars were also found, in one of which mortar with wattle impression was found and assumed to come from an upper storey.

Vast numbers of finds came from the site: bones, metalwork, and pottery. Badorf pottery and enamelled metalwork suggest a late eighth-century date for its earliest occupation which appeared to continue unbroken into the tenth century.

Bibliography: Gensen, Hellwig, and Kütke 1973; Gensen 1981, and in Roth and Wamers 1985.

Inhabited or Uninhabited?

There is no question of these four sites, which have been more extensively excavated than most enclosed Carolingian sites, being interpreted as uninhabited refuges. Yet this is precisely what many unexcavated enclosed sites are said to be.

In chapter two we saw the intellectual roots of this idea going back to the previous century. The idea has been strengthened by the topographic theory of Gauert (1965b), who divides the physical structural topography of royal palaces into three parts: residential palace complex, agricultural estate, and fortification. The latter, Gauert argues, need not have been present but the agricultural estate was essential. However, even given palaces with all three components, Gauert argues that they need not coincide physically. Thus, topographically seen, the individual components may well be separated one from another by a kilometre or more. Now, clearly the separation of agricultural estate and lordly residence was not only possible, it was a necessary fact. That is to say, important nobles had numerous properties scattered about the countryside; we have already noted possessions in the south of France held by northern nobles for the sake of their wine, and perhaps oil and herbs. They also had numerous, sometimes thousands of dependants of varying degrees of servility. They could not all have lived huddled around his home. An estate in Frankish terms was never a strictly

nucleated settlement and the very existence of something we would term 'palace' presupposes a level of exploitation that allowed nobles and kings to reside apart from those who laboured for them.

The separation of a fortification from the palace complex is an altogether more dubious proposition. Gauert's intellectual dissection of the components of a palace and his allowance that they might be spatially separated has provided a theoretical basis for accepting the interpretation of refuge forts. Indeed, to say that the palace fortification might be purposefully built some kilometres from the palace itself is to say that refuge forts were constructed.

The empirical evidence to support the idea is exceptionally poor, although circumstances conspire against proof if refuges were indeed built. Given that they were theoretically uninhabited, there should be little dating evidence. It would take extensive excavation to reveal conclusively that the site had not been occupied, and be able to date its construction. The ability to connect it with an undefended settlement of contemporary date is the second difficult task.

Let us now turn to two examples that are commonly claimed to be refuges: Unterregenbach and Chèvremont.

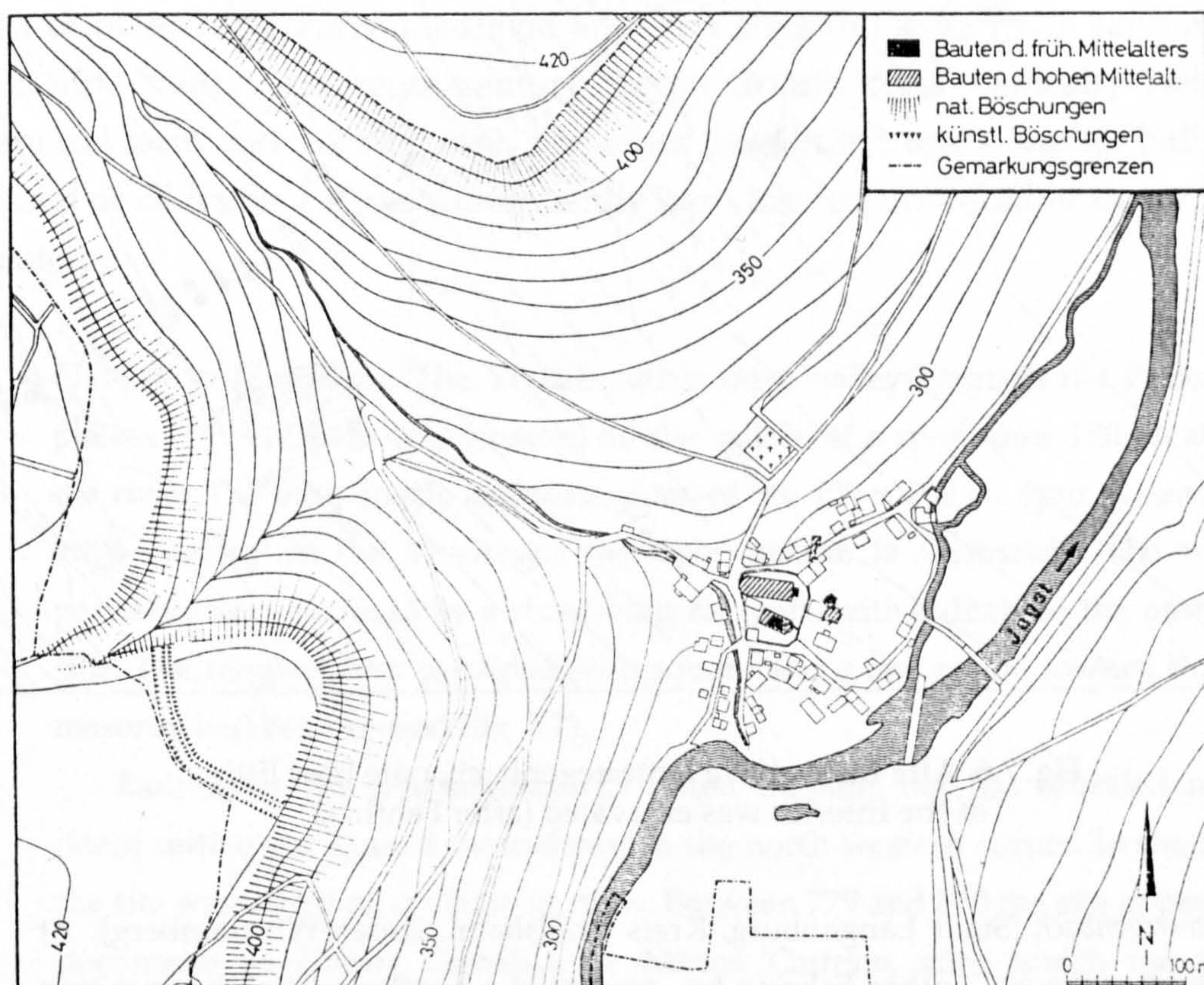


Fig. 7.5 The little village of Unterregenbach lying under the Alte Burg hillfort. In the village an early medieval predecessor to the present church was excavated, and a few buildings nearby.

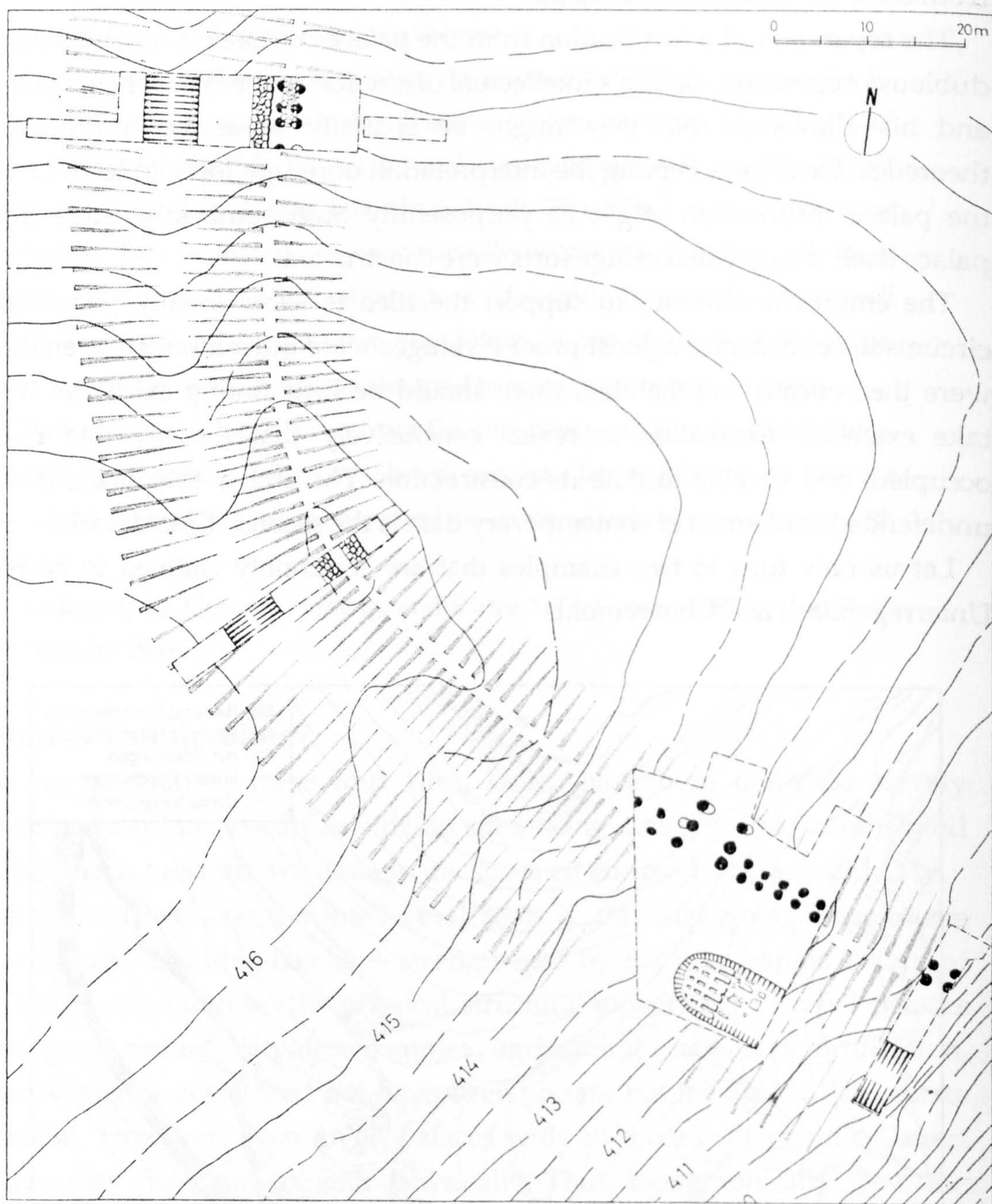


Fig. 7.6 Alte Burg above Unterregenbach, note how little of the interior was excavated (after Fehring).

Unterregenbach (Stadt Langenburg, Kreis Crailsheim, Baden-Württemberg). At Unterregenbach Günther Fehring has excavated a number of buildings within the little village and sectioned the ramparts of a hillfort situated on an overlooking spur some 400 m. to the west (figs. 7.5 and 7.6). The fortification is of at least three phases, one timber, one drystone, and one of mortared stone.

Dating is far from clear but Fehring places the use between the eighth-ninth century and the tenth-eleventh. Only a tiny percentage of the interior was sampled. Nothing was found. Within the village the excavated remains of at least two churches are clear. The earliest sacred building is dated to early in the ninth century. Outside the walled churchyard there are fragments of other buildings, including a solidly built hall measuring 7 x 9 metres internally. This late building, which overlay at least two earlier buildings, is dated to the tenth century. There is no textual evidence to help us with this early history of Unterregenbach.

Bibliography: Fehring 1973 a. and b.

There is really too little evidence here to make more than guesses. The tenth-century building only 30 m. from the churches might have been owned by the local lord, but then it need not have been his residence, and equally it might have formed part of the ecclesiastical complex. The building's predecessors could be anything at all; we cannot make more of the plan than a few disjointed walls. The possibility that the entire area enclosed by the rampart was uninhabited is scarcely strengthened by the minimal amount of investigation undertaken. The enclosed area of other large early medieval enclosed sites in Germany is seldom densely built up. Ninth- and tenth-century Saxon 'round forts' typically hold only a manorial farm and nothing else. The other possibility, that a timber hall has been missed or all trace of it has subsequently been eroded downhill is, to my mind, the greater.

Chèvremont (Belgium). The Vesdre cuts a deep valley through the Ardennes plateau. Chèvremont was situated on the saddle of a spur some 120 m. above the river. The east, south, and west sides of the 400 x 200 m. spur plateau fall away sharply to the Vesdre or the little stream, la Casmatrie. The whole perimeter was enclosed by a stone-built rampart, with a ditch on the northern side. The rampart was provided with square and a few round towers; Roman masonry had been reused (fig. 7.7).

Excavation has not satisfactorily dated the site, but has revealed rather dense settlement against the rampart in the north-western corner. In the 1050s the site was called an *oppidum* in ruins. Between 779 and 870 the site appears in documents as *Novum Castellum* or *Novum Castrum*, after which the name changed to Capremons. The site was the scene of siege and military refuge throughout the tenth century.

Bibliography: Werner 1980.



Fig. 7.7 Chèvremont (after M. Werner).

Werner (1980) draws attention to the remote situation of Chèvremont, amid a number of important royal villas and palaces. Given its later military history, Werner concludes that Chèvremont was meant, from its inception, as a refuge fort, serving Jupille and Herstal. However, there is no evidence for early Carolingian kings having built forts anywhere near any of their other palaces. Moreover, Chèvremont first appears in documents when St. Mary had possession of eleven named properties confirmed. The same charter of 779 reveals that the monastery there was founded by Pippin II. There seems little reason to interpret the rampart as anything other than a massive monastic *vallum*. Such powerful enclosure of religious houses was common practice for the period (Samson forth. a; James

1981). The name, 'New Castle', was a very common one throughout the Merovingian and Carolingian period, applied equally to monastic foundations and villages (see Rouche's map, Fig. 2.12, in chapter two for the many *novi castelli* of Aquitaine).

In fact the majority, if not all of the supposed cases of undefended palace or noble residence and refuge fort fall well short of that level of proof. More often than not the two sites cannot be shown to be contemporary and the enclosed site will only have been sampled on a small scale, if at all.

It is my impression that in most cases of presumed paired refuge and manorial estate the 'fort' yields older archaeological evidence than the still-occupied village which is meant to be the successor of the originally contemporary undefended manor. Stray finds, a few patternless post-holes, or the odd grave in the village are sometimes taken as evidence of a settlement which is pushed backwards in time from known medieval documentary records. It is my impression that an alternative which sees the enclosed site as an earlier ninth- or tenth-century settlement that subsequently moved down into the valley in the eleventh century is equally supportable by the scattered, partial evidence of most postulated sites. Many German archaeologists would disagree with this interpretation vehemently.

One last complication is certainly worth noting, for in it may lie the key to much of the understanding of German Carolingian villas. The complication is one of vocabulary. German archaeologists talk freely of the relationship of *Burg* and *Hof*, but this might be translated as either 'fortification and manor' or 'castle and estate'. The difference is not small, for the former implies an empty refuge and takes little notice of dependent peasants whereas the latter implies a marked spatial and architectural distinction between lordly residence and peasant village. Seeing Eringsburg as a lordly residence and Iring as a village of dependent peasants has much to offer. It allows us to dispense with the refuge theory and it is better suited to explaining the charter evidence: it is parts of the agricultural estate that are given away in these documents, primarily to the Church, and the noble's residence is understandably absent from them.

German archaeologists, however, do not think in terms of these differences, not only because the same words cover a multitude of meanings, but because they are quite accustomed to the idea of refuge forts and do not necessarily sense the difference between nobles living in their 'forts' or beside them. Indeed, so firmly entrenched is this duality that East German scholars adopted it. Hansjürgen Brachmann (1984, 494) concluded that the *Burg* in proximity to the *Hof* was 'a constituent element in the Frankish form of feudal development' and that when the Merovingians took over the Roman fisc 'fortified *villae rusticae* and forts by

villas became the strongholds of warring Franks.' But we have already seen that late Roman and Merovingian villas were no strongholds and that fortification seemed to play no role in the residences of nobles, warring or not. Brachmann's nonsense can perhaps be excused on three counts. Firstly, sufficiently little exists in the literature (and almost none in German) about the nature of fifth- to seventh-century Gallo-Frankish villas to facilitate wild speculation. Secondly, Brachmann's research was almost exclusively confined to areas east of the Rhein; we all make the occasional, outrageous remark about things of which we know little. Thirdly, he only followed what the excavator (M. Werner) said about Chèvremont.

The Curtis Question

The so-called *curtis* question (a debate exclusive to German academic circles) began at the turn of the century when Stephani (1903) wrote a book on the earliest German dwellings, Rübel (especially 1904) discussed Frankish institutions and particularly his views on the Carolingian conquests of the territory east of the Rhine, and Schuchhardt (1888-1916) produced an atlas of the earthwork sites in Lower Saxony. The influence of Schuchhardt and Rübel has been particularly great and expressly recognised by many authors.

Rübel argued that the Carolingian conquests were maintained by a series of fortified strongholds, particularly along important overland and riverine routes. This idea has been taken up by a number of later writers (eg. Görich, Wrede, Stengel, and Nitz) and their theories and influences will be dealt with more fully shortly. Schuchhardt had noticed that exactly in the areas of earliest Carolingian conquest (Hessen for the most part) and outside his own Saxon area, earthwork sites of rectangular plan were known. These were thus seen as the regular strongholds which Rübel postulated and were designated *curtes*. The name was applied on the basis of their interpretations of the *Brevium Exempla* and on the argument that some of the archaeological sites could be equated with sites termed *curtes* in the documentary sources. One such was Altscheider. The RFA record for the year 784 that Charlemagne celebrated Christmas in *villa Liuhidi* (Lügde), which was *iuxta Skidrioburg*; in 889 we find *Schidara* as one of several *loci* being donated *cum curtilibus, aedificiis*, and so on (Dolling 1958, 68). Schuchhardt interpreted *Schidara* to be a *curtis* and to be the equivalent of modern Altscheider, while *Skidrioburg* was modern Herlingsburg lying near Lügde and seen as a Saxon *Volksburg* and a Carolingian royal, military fort. At Altscheider Schuchhardt carried out some excavation on what later proved to be two successive enclosures rather than the double enclosure Schuchhardt thought he was dealing with. The earlier phase was an oval-shaped enclosure 150 x 120 m. surrounded by a V-

shaped ditch which yielded pottery dated by the excavator in 1950 to the beginning of the ninth century. It was cut by a later rectangular 270 x 170 m. enclosure, similarly defended by a V-shaped ditch, but alas without dating evidence (von Uslar 1964, 45-7).

Dolling (1958, 68-9) amassed all the mistakes and inconsistencies in the arguments of Schuchhardt to show that Altscheider was not a two-part enclosure which was called a *curtis* by contemporaries. Schuchhardt preferred to see such sites as being composed of two enclosures: the *curtis* proper for the commander and a lower *curticula* for the garrison – the vocabulary was drawn from the *Brevium Exempla*, the interpretation from Rübel. Although this was his preferred scheme, it should not be taken as disproving his hypothesis simply because the site only ever had one enclosure. Indeed the *Brevium Exempla* reveals *curtes* without an attendant *curticula*. As to Dolling's other criticism, the charter recording the king's alienation of possessions in half a dozen *loci*, including Altscheider, with their *curtilibus*, buildings, and fields both cultivated and uncultivated, admittedly does not explicitly call Altscheider a *curtis*, but Schuchhardt's acceptance of the site as such is not a crime. The king was clearly granting agricultural estates or portions of them, so that the use of the term *locus* should only be taken to mean that Altscheider was perhaps only a rather modest villa.

The fatal mistake made by the followers of Schuchhardt in the 'curtis question', was to use the word *curtes* as a technical archaeological term for all sites of rectangular or shield-shaped enclosures, preferably with one or more attached annexes. Von Uslar (1964, 61) even claimed that this was defensible if only as a heuristic device. Five years later, however, he ate his words when he published an article entitled 'Abschied von der Curtis'. He was forced to take his leave of the term because of the stinging criticisms levelled at him, particularly by Gauert (1965b), who reviewed his book, and by Hinz (1967). The major criticism was simple and not new: *curtes* appear or rather abound in the historical sources as a generic term for manors. Dolling (1958, *passim*) revealed (see above, chapter 5) how *curtis* and *villa* were synonymous in the barbarian laws. The best example of such an equation is in the title of the famous capitulary issued by either Charlemagne or his son Louis the Pious, *Capitulare de villis vel curtis imperialibus*. If the term meant anything in a technical sense, it meant agricultural manor. The fortification of agricultural estates has been seriously doubted and Hinz (1967) suggests that the *Brevium Exempla* most certainly does not depict fortified villas. Gauert (1965) further argues that royal Carolingian palaces were not even found on naturally defensive topography. As a result the terms *Burg*, thought to be less

specific, and *castrum*, borrowed from contemporary sources, have been used to describe the rectangular and so-called shield-shaped enclosures. Because no unenclosed sites – other than ‘villages’ – have been excavated and because the new trend divorces the term *curtis* rigidly from the earthwork sites, the prevailing belief is that no *curtes* have been excavated. Hinz (1967, *passim*) urges the excavation of a *curtis* as an urgent concern, to which von Uslar (1969, 153) agrees. Such a view could only be born of confused logic.

Most of the confusion has been self-inflicted on those concerned with the ‘*curtis* question’. In large part it centres on the definition of enclosure and fortification and the assumption that it must be a case of all or none – all villas must be enclosed/fortified or none may be or that all enclosed sites must be classed together. It will be argued here that many, if not most Carolingian villas were indeed enclosed, but that only with a very wide definition could these be said to be fortified.

We must begin by accepting that an enclosure of whatever kind has not been found at all sites. The excavator of Bodman does not believe that the hall was ever surrounded by a wall, although set on a prominent little eminence. In Zürich the late Roman *castellum* walls and towers were clearly no longer standing and the same is probably true of Kirchen, although there the excavation was of poor standards. At Zullenstein, again the Roman fortifications were built over, although if a Carolingian enclosure were present the later medieval castle defences would have removed any trace entirely. At Aachen there is surprisingly little evidence for an enclosure or rampart, for unlike Paderborn, where the cathedral ‘immunity’ precinct coincides with the Carolingian wall circuit, the postulated palace precinct at Aachen does not even remotely coincide with the cathedral ‘immunity’. In further distinction to Paderborn, at Aachen there has only been one tiny dubious section of wall found.

At Frankfurt there was found a substantial stone wall, although this has been interpreted as a later addition, so that the original Carolingian palace built under Charlemagne would have been unenclosed. Otherwise only at Paderborn was a very large stone rampart wall with towers discovered. Such an enclosure wall could not be denied a defensive purpose.

At Ingelheim there is no evidence or even reason to think that the palace was enclosed, but then the palace precinct in effect closed itself with continuous buildings. The western facade reconstruction of Aachen is very striking, although perhaps not quite so forbidding as the eastern facade of Ingelheim. In both instances a gate-hall carefully controls access into an inner courtyard whence the royal hall is attainable. Importantly, although access to the minster at Aachen from

outside was possible, it was not possible then to enter the inner courtyard except by the *porticus*, which in any case ultimately led back to the gate-tower and was thus no independent means of entry. Such rigid control forces us to the conclusion that the courtyard could not be gained by simply wandering around to the eastern side of the palace complex. Thus we must see Aachen as having been enclosed in just the same way as was Ingelheim. Although the evidence from Samoussy leaves much to be desired perhaps it too should be added to this group which formed an enclosure by continuous buildings or with gaps filled by joining stretches of walling. Such a method of enclosure was complex and perhaps uncommon. It is no coincidence that Aachen and Ingelheim were chosen by Einhard as two of three palaces constructed by Charlemagne that were exceptionally noteworthy architectural projects.

Table 7.1 Buildings of royal villas in the *Brevium Exempla* (after Hinz).

| Fisc name <i>Brevium exempla</i> chapter | Asnapius c. 25 | c. 30 | c. 32 | c.34 | Teola c.36 |
|---|-------------------|-------|-------|------|---------------|
| <i>sala, domus,</i> <i>casa regalis</i> | 1 | 1 | 1 | 1 | 1 |
| <i>camerae</i> | 3 | 2 | 2 | 1 | 2 |
| <i>solariae casae</i> | x | 2 | | | 1 |
| <i>cellarium</i> | 1 | | 1 | 1 | 1 |
| <i>torcularium</i> | | | | | 1 |
| <i>porticus</i> | 2 | | 2 | | 1 |
| <i>mansiones</i> | | | | 3 | 3 |
| <i>aliae casae</i> | 17 | 8 | 2 | | |
| <i>stabula</i> | 1 | 1 | 1 | 1 | 3 |
| <i>coquina</i> | 1 | 1 | 1 | 1 | |
| <i>pistrinum</i> | 1 | 1 | 1 | 1 | |
| <i>horrea</i> | | | 2 | | |
| <i>spicaria</i> | 2 | 5 | 4 | 2 | 1 |
| <i>granecae</i> | | 3 | | | |
| <i>scurae</i> | 3 | | | 3 | 2 |
| Total | 33 | 24 | 17 | 14 | 16 |
| <i>curticula</i> | x | x | no | x | no |
| <i>tunimus (curtis)</i> | x | x | | | x |
| <i>tunimus (curticula)</i> | x | x | | x | |
| <i>sepis (curtis)</i> | | | | x | x |
| <i>murus</i> | | | x | | |
| <i>porta lapidae</i> | x | | x | | |
| <i>solarium (porta)</i> | x | x | | x | |
| <i>porta lignea</i> | | x | | x | x |

The most important piece of documentary evidence to set alongside the archaeological, is the *Brevium Exempla*, which describes five royal estates. Hinz

(1967) has summarised the evidence concerning the enclosures mentioned in tabular form (table 7.1). Hinz claims that in two cases there was no *curticula* and thus 'almost half the sites' were without. Better to say simply that in two of the five there was none recorded. A better 'statistic' would be that in all instances a main *curtis* enclosure wall was recorded. However, any conclusions drawn from the *Brevium Exempla* will necessarily involve serious wrestling with the interpretation of the terminology used. Although Hinz did consider the terms in some detail and in conjunction with archaeological evidence, there is still scope for improvement on his interpretations.

Curtis is found generally as a synonym for villa. In chapter five it was noted that legally there was a distinction between inside and outside a *curtis*, but this appeared to imply no more than on or outside the estate property and not, as translators have often preferred, inside or outside a courtyard. In that way *curtis* was acting as a synonym for villa. It could be argued that buildings found *infra curtem* (as in cap. 30 of the *Brevium Exempla*) mean no more than buildings found 'on the villa'. When the *curtis* is described as 'enclosed', it is done so either by *circumdata* or *munita*, while the *curticula* was described as 'enclosed' by *interclusa*. The fact that the *curtis* and *curticula* are themselves never described as being themselves a *tunimus* or a *sepis*, or *ex lapido* or *ex ligno* is significant. The sheer unwieldy length of such expressions as *curticulam interclusam cum tunimo strenue munitam* in cap. 32 is proof that the term *curticula* did not refer to the actual enclosure wall itself.

We now reach the semantic level where we must ask, could a *curtis* and *curticula* exist without their enclosures. In the case of a *curticula* this would appear impossible, for it only ever appears as an area, such as in cap. 25 where it seems to be an ornamental garden, which is enclosed. The term was to be used well into the Middle Ages meaning precisely 'a small enclosed yard'. The *curtis*, on the other hand, as has been repeated often, was a generic term for something more than the *cohors* - yard - from which word the term developed. *Curtis* is thus like our 'court', it was extended to something beyond the original, literal, physical object. The German *Hof* is almost equivalent, for it too means courtyard and farmstead. *Curtis*, however, had become more closely connected to the manor as a whole, for I cannot find any use of the term as unambiguously meaning courtyard. I believe that the *curtes* in the *Brevium Exempla* would not have been designated by a different term had there been no enclosure; a *curtis* would remain a *curtis* without a *tunimus* or without a *sepis*. As all five *curtes* of the royal fisc in the *Brevium Exempla* appear as enclosed and given the archaeological evidence of enclosed space created by contiguous buildings at Aachen, Ingelheim, and Samoussy, and the more

conventional enclosure of Paderborn, we may conclude that it was indeed common practice, although not indispensable, as we saw of Bodman, Zürich, Frankfurt in its earliest phase and probably Zullenstein.

Dolling (1958, 66) rightly stressed that *circumdata* and *munita* were used interchangeably in the *Brevium Exempla* and were better translated as 'enclosed' rather than 'fortified'. The nuance of 'protected' might well be considered, in part thanks to the derivative verb *munire* but also the additional adverbial description *strenue* which appears twice. This stands out against the *optime*, *bene*, and *ordinabiliter* adverbs used of the construction of various buildings. In cap. 30 we find the addition of *spinis* which can only have been added to make ingress more difficult.

Now the vexed question of the enclosure itself: what do the terms *murus*, *tunimus*, and *sepis* mean? *Murus* is clearly a stone wall. It appears only in the instance of Treola, which also has a stone gateway. Only Asnapius and Treola had stone gateways; at the other three fiscal *curtes* they were wooden. In view of this exalted position of Treola and Asnapius, it is important that the *curtis* at Asnapius was *strenue* enclosed by its *tunimus*. It can be suggested that a *tunimus* was a lesser construction than a *murus*, first because in two cases it is accompanied by a wooden gateway, which seems unlikely if a *tunimus* was a stone rampart. Secondly in two instances additional features are described as accompanying a *tunimus* which again seems unlikely if it were considered something like the rampart of Paderborn. Thus a *tunimus* was less desirable than a *murus*, but that found at Asnapius was more impressive than normal for it enclosed the villa 'strongly'. The *tunimus*, whatever it was, was clearly more respectable than the simple *sepis* that was found at the *curtes* of cap. 27, 28, and 29, which were all dependent on Asnapius and where there was no royal domestic dwelling. Only one *curtis*, of an unnamed fisc in cap. 32, is described as enclosed by a *sepis*; the other four by a *tunimus* or *murus*.

What a *tunimus* was, we cannot know. It does not appear to have been an extremely common word. Interestingly another instance of its appearance is in a polyptych of the Abbot Irminon and thus close to the *Brevium Exempla* in time and purpose. It appeared in the Bavarian laws and was later glossed as *hovezun* (du Cange, *sub* 'tunimus'). *Hofzaun* brings us little closer to a solution and the inclination to see it as a 'fence' (*Zaun*) is best avoided. In an attempt to make the evidence fit the archaeological picture, the *tunimus* has been interpreted as an earthen rampart, probably with a V-shaped ditch, and perhaps with a timber facade. It is possible that this interpretation is strengthened by the phrases *desuperque spinis* and *desuper sepe* (caps. 30 and 34). Dolling, who preferred to

underplay all possible fortification interpretations chose *desuper* to mean 'moreover', although the *solarii* of the *Brevium Exempla* are invariably found *desuper* – on top of – gateways and buildings. If the *tunimus* were an earthwork, it is perfectly reasonable to find a thorn-bush hedge or a *sepis* on top. The only feasible alternative for *tunimus* would be a palisade. Preference for an earthwork depends in part on the interpretation of *sepis* as a fence, for then a fence on top of a fence makes little sense.

In chapter four, *sepis* was mentioned briefly in reference to the possible enclosure of Merovingian villas. Although it could refer to a hedge, it is clear that *sepis* in the works of Gregory of Tours refers to fences. The barbarian lawcodes were very clear about the fines for people who cut, stole, or pulled out the three posts (*palum*, *cambortus*, or *virga*) which make up the *sepis* (*Lex Salica* 34.1, *Lex Ribuarica* 72.1). The minimum height of a *sepis* was that which reached the chin, for an animal might impale itself on the *sepis* (*Lex Ribuarica* 73.3). The liability of an impaling on a *virga* of the *sepis* is discussed in *Lex Ribuarica* 73.4. Dolling suggested that these fences or palisades enclosed Frankish farmsteads and villas, although from the lawcodes we may only infer that they enclosed fields. Since Dolling wrote, however, the archaeological evidence is slowly accumulating to show that a communal village palisade fence, or individual 'magnate farm' enclosures were indeed common, perhaps the rule. The *tunimus* and *murus* of the *Brevium Exempla* can surely be seen simply as more luxurious examples of something similar.

The evidence, linguistic, archaeological, and textual all suggest that villas and *curtes* were regularly enclosed by some means. At the end of chapter five I discussed the importance of enclosures as marking and legally protecting private property and as a means of controlling slaves. These roles, I might add, are the only ones documented. They are the only ones explicitly recognised in lawcodes; they are the only way they are depicted as functioning in anecdotes related for other, didactic reasons. As defensive protection they are never documented.

Whether or not these are to be understood as defensive will not be satisfactorily answered, for the debate will centre on what is understood by defence. Intimidating thieves and cut-throats may suffice to be accepted as a defensive measure to some. Certainly in the eyes of contemporaries, villas were seldom considered to be fortified, at least in the sense of capable of withstanding the attacks of armies. Bachrach once countered the imaginative theory that the introduction of the stirrup in the reign of Charles Martel was a revolution, resulting in the 'rise of feudalism', in part by showing that most of his campaigns and those of his sons consisted of sieges of cities. Just as in Merovingian times, cities were the main military objectives. Villas almost never appear in any war.

accounts, with the major exception as an assembly point. Thus Herstal and Aachen served as departure points for campaigns against the Saxons. Paderborn was without doubt in a unique position as a major royal villa, but lying close to an active military frontier. Here we see the army assembled when Pope Leo arrived to meet Charlemagne. The mention of the 'camps' given in the poem describing the meeting of the two men gives the impression that the army was encamped in tents beyond any defences offered by Charlemagne's palace enclosure.

The Mania for Regal Explanations

German scholars working with archaeological, historical, topographical, and place-name evidence all impute royal Carolingian initiative to a wide range of phenomena they study. Attempts have repeatedly been made to accredit certain place-names with royal property and settlement. This is convincing in the case of Königshagen, but is derisory in the case of -dorf names, or names with cardinal directions – north, south, east, west – as prefixes. That most of these theories are based entirely on fantasy is revealed by the place-name chapter of the otherwise respectable work of Heinemeyer (1971). He imputes Frankish royal policy to the settlement of Franks in equal-sized hamlets, bearing -hausen names, in the region of Kassel between 650 and 750. Now, imperial property here is first known *only* from the tenth century; the suggestion of Frankish political hegemony before 700 is highly questionable; imperial 'colonisation policies' seem anachronistic for the early Middle Ages; empirically there is not a single recorded name in the region *before* 750; and the sum total of archaeological evidence for the region in the period 650-750 is a *single* sherd of *Knickwand* pottery. In short, there is no evidence of anything for the period.

Attempts have been made to see Frankish royal and imperial programmes of frontier colonisation with warrior peasants from the shape of modern village plans (Nitz 1963; 1983). That English archaeologists should quote this work as worth serious consideration is upsetting. Years of detailed archaeological investigation of Alfredian *burhs* has allowed us to detect planning in town and street lay-out, but claims put forward are very circumspect. Nitz's work is as if one were to claim that all 'ribbon'-villages of the Welsh Marches were purposefully founded military peasant colonies by Offa in order to protect Mercia.

Attempts have been made time and time again to interpret documented institutions as royal Carolingian inventions. Thus the *marca* is often said to be an invented administrative division or an agricultural measure designed to improve agricultural output or to standardise the levy of soldiers or taxes. Because *marca* derives from 'boundary', historians have happily accepted an administrative

origin, despite the clear variety of meanings it has depending on context. Thus it was easily applied to huge regions of the kingdom such as the East-mark. More often than not it has the sense of private property, in the same way Merovingian charters spoke of villas with all their *finēs*. More than one proprietor could own land within a *marca*, just as in Carolingian Gaul we find multiple owners within a *villa*. These need not then be interpreted as administrative areas, for *marca* could simply have come to mean something like 'village', or a former estate could have become subdivided leaving its former unity only as a memory in the term *marca*. Another alternative, so seldom remembered, is that medieval land rights were nestled in a hierarchy, so that one could 'own' land that 'belonged' to someone else. There could be several proprietors of various pieces of land within someone else's *marca* or villa, to whom they had obligations, whether rent, render, services, or labour, or any combination of them. This is the same complication discussed at the end of chapter two, the same problem that confused Herlihy into believing that he saw people selling other people's property.

Given the background in which Charles Martel and Charlemagne are seen to seize most of the countryside east of the Rhein for themselves, introduce a new legal, social, religious, and economic administration, reorganise land-holding and land divisions, even field lay-outs, plant colonies of Franks in literally thousands of settlements in planned villages, create communication networks, and found monasteries, build palaces, and plant towns and quarters for long-distance traders, it is not surprising that archaeologists have thought they detected the hand of these great kings in everything they have dug up. Even *Reihengräber* cemeteries become the burial grounds of the *Königsfreie*.

Nowhere is this more true than of the *Burgen*. Anything that may be interpreted as fortified is invariably attributed to Frankish royalty. The only variation is whether the official, to whom it is seen as entrusted, remained loyal. Schuchhardt first suggested the typological distinction for Frankish hillforts that he saw as having been built against the Saxons. The idea was adopted and expanded by Willi Görich (1936/48; 1951) who suggested that a whole series of hillforts in Hessen were shield-shaped and could be attributed to the Frankish monarchy. It supposedly had these forts built as military strongholds along major important routes, acting much as Roman marching camps did, stationed a day's march apart. The army could thus quickly and safely get to the frontiers of the empire to deal with an enemy. Görich's thesis has been adopted by many and has been made to fit nicely with Schlesinger's 'constitutional' history of early medieval Germany. *Burgen* are automatically ascribed to royal initiative because the construction of fortifications is taken to have been a royal prerogative.

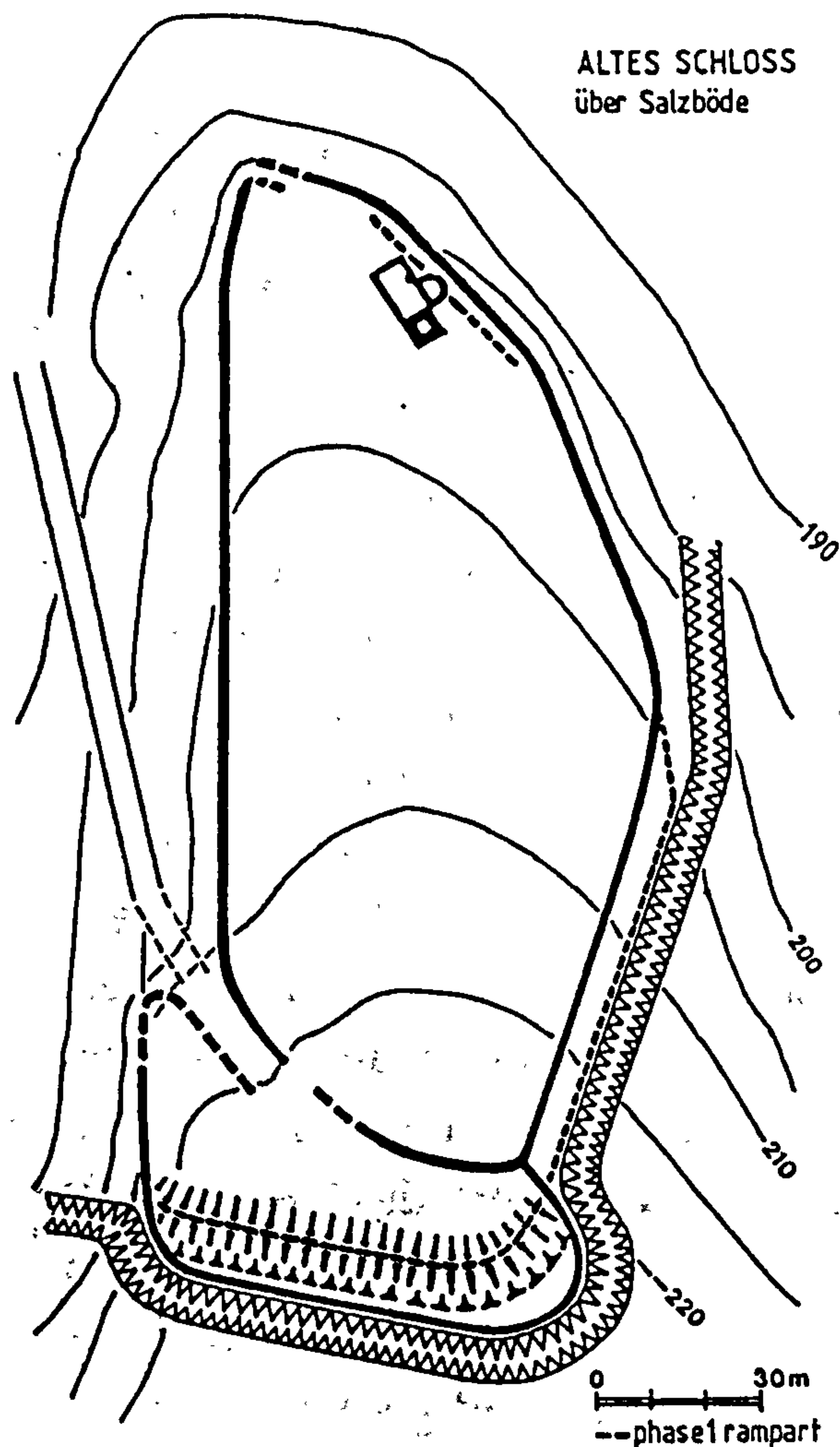


Fig. 7.8 Altes Schloß, Hessen. Interpreted as a military marching camp by the excavator, interpreted by the author as a small manor.

Befestigungsrecht is unquestionably ascribed to the monarchy (see Fehring 1987 for an example of unquestioning acceptance).

No aspects of Görich's arguments stand up to close scrutiny. Firstly, there is no typological unity of these sites, despite the claim that they are 'shield-shaped'. Gronauer Altes Schloß, the site which started Görich on his thesis, is typical (fig. 7.8). For most of its circuit it follows the natural advantage of the local topography. When it is forced to abandon the natural slope, it encloses a maximum area for a minimum length, that is, it is roughly semi-circular to oval. Only the most irregularly shaped sites cannot be counted as 'shield-shaped' and sites under 3 ha. are rarely anything but roughly circular, oval, or rectangular. Secondly, almost all early medieval fortified sites are located on major routes. Thus Schwarz (1975, 384-6) notes that for north-eastern Bavaria early medieval hillforts almost invariably lie near long-distance routes. Saxony was no different (Jankuhn 1976, 370). Indeed

in Holstein just north of the mouth of the Elbe, Jankuhn notes the preference of siting on the edges of settlement basins and on major routes. The *Heerweg* practically runs directly into the small *Ringwall* at Kaaksburg. Such a preference, especially for major waterways, can be noticed among the Carolingian palaces and villas, as in the case of Ingelheim, Frankfurt, Paderborn, Salz and *Querfurt* (literally 'ford cross'), just as it was for Merovingian and Gallo-Romans before them, although these were most emphatically not built with military functions in mind.

It is scarcely necessary to summon a strategic explanation for these settings. Major routes also tend to be fertile valleys, the domination of which was the source of local power and authority. Such routes are also the product of geology that makes for ideal topography for defended sites: hills and spurs, well-drained and dominant terraces and plateaux, and all near fertile arable land. The question of strategic importance seems often unnecessary and anachronistic. In any case, if Görich's shield-shaped forts were really meant to protect the routes they were not always well positioned locally. In the area around Marburg, along the Weinstraße, the route cannot actually be seen from Gronauer Altes Schloß, Hundburg bei Oberrsphe, or Christenberg. At best the Lahn river valley can be seen from the latter two, but then the nearest point from the Christenberg is well over three kilometres away. The Gronauer Altes Schloß is in an even worse situation. Lying some four kilometres up the Salzböde stream, away from the Lahn, it overlooks nothing. In fact, even today the access road goes only a little farther up the tiny valley. Better hidden from the Weinstraße it could not be. To serve as an *Etappenstation* it would have involved at least an hour's march by foot off the Weinstraße, and of course another hour's march the next day to rejoin it again.

When Schwind (1984, 39-40) suggests that a criticism of Görich's thesis must await the systematic consideration of individual sites, he implies that insufficient dating evidence is available to us. I disagree. There are several indicators that the chronology of building and occupation among the various sites in question is spread out throughout the whole of the eighth and ninth centuries. There is no suggestion that they fall into distinct and short time spans. For example, the pottery types found at the Höfe bei Dreihäusen largely parallel those found at Christenberg, which has now produced the standard type collection. The few absent types are thought to be the earliest at Christenberg, so that the Höfe is considered to have been first occupied one or two decades later. The Gronauer Altes Schloß, from its pottery, is not thought to have been occupied until the middle of the eighth century, thus later than Höfe. The Hünenkeller bei Lengefeld seems to start at the mid- to the beginning of the last quarter of the eighth century

and is perhaps slightly younger than the other three. This undermines the suggestion that a whole system of fortifications was ever conceived by the Carolingians, for they seem to have developed ad hoc. Moreover the only *castra* we ever read of being built by Carolingian armies was on the border or in enemy territory, such as that on the Lippe in 776 or that on the Elbe and Saale in 806; in 809 Esesfeld was built 'on the other side of the Elbe'.

Finally the whole question of the royal prerogative to build fortifications is in serious need of reconsideration. It should be the subject of a whole paper, but suffice it to say here that historians and archaeologists tend to make the mistake in equating the royal claims of a *Befestigungsrecht* with the interpretation of all fortifications as royal. There are at least four problems that are usually misunderstood. Firstly, the right was a historical development, it was not a divinely ordained right of monarchs. That is to say that Frankish kings did not always have the right, did not always claim the right, did not even think in terms of rights in building defences. The first evidence we have is Charles the Bald's statement in the *Edict of Pîtres*¹ in 863 that no one should build fortifications without his permission and that adulterine castles would be destroyed. It seems unlikely that Charles the Bald's predecessors ever gave the problem any thought; there is every reason to suspect that Charlemagne never claimed any such right for himself. Moreover we have the case of Alamannic *Burgen* and hillforts in Saxony when there were no kings and thus no regal rights. When Boniface came to Amöneburg in 721 and found it in the hands of Dettic and Deorulf,² it seems unreasonable to assume that they held the place as a military station for Charles Martel and not in their own right. Why would Boniface have had to negotiate directly with them, why would Charles Martel send out men who 'practice paganism under the guise of Christianity'? And why the oddity of a pair of brothers holding the fort if it were not that they jointly inherited it from their father? And what of Büraburg and Christenberg, dated to just before 700? It seems quite possible that the large enclosures around these places were built before Charles Martel re-exerted the long lost Merovingian hegemony over Hessen.

Secondly, claiming a right does not give one a right. Just because it was the king that announced that he had the right to refuse permission to build fortifications does not mean that he was heeded. Historians too often accept that a king's word was law, and if it was not that it should have been. Thirdly, even if it was generally accepted and agreed that the king indeed had the right to demand that someone not build a fortress, it did not necessarily stop nobles from doing so.

This point is recognised, but simply because it is the negation of the royal prerogative that everyone assumes existed. Thus Schwarz (1975a, 389-91) gives the

¹ Translation in Fournier 1978, 267-8.

² Tolbot 1954, 42.

examples of Castell, first mentioned in 816, which he associates with a noble family in Bavaria, and the 'village of Iring' mentioned in 822. Above the village is a hillfort, Eringsburg, which is assumed to be an early medieval site, presumably the residence of the Frank Iring. Schwarz suggests that the nobles at Castell and Eringsburg had usurped the royal right of fortification very early. Early is an understatement given that the examples come half a century before the first royal claim to control the construction of fortifications had been made in western Europe.

Fourthly, and most important of all, kings only claimed that they had the right to *deny* permission for the building of fortifications. Historians have long misinterpreted this to mean a monopoly. Coulson (1973; 1976) has shown that this was hardly the case. It went without saying that those friendly and loyal to the king would not only build fortifications, but would not even bother to ask for permission. Asking for permission was demeaning, and not expected if relations were good and the nobility great.

Enclosed Sites as Simple Villas

East of the Rhine important Carolingian settlements of various nature were all enclosed, whether large monasteries, royal palaces, villas, episcopal centres, trading *emporia*, or towns. Some of these are hard to mistake, such as the monasteries, royal palaces, and coastal *emporia*. The remainder are less obvious. German scholars prefer to see almost everything else as royal forts, garrisoned by soldiers, administered by dukes, or misappropriated by officials. There is little space given to simple *curtes* of nobles.

In chapter two the use of the term *castrum* was investigated. It continued to mean camp and appears frequently in the *Royal Frankish Annals'* recitation of Charlemagne's Saxon wars. Military camps and forts were clearly built during campaigns. It continued also to have the meaning of strongly enclosed, and as in Merovingian Gaul, the name *Novum Castrum* continued to be popular for new small town foundations - *vici*. Most of the important town-like settlements of Carolingian Germany, like Eresburg, Büraburg, Amöneburg, and Würzburg, were called *castra*. These were the sort of sites Boniface chose for bishoprics and had to argue were 'urban' when corresponding with the pope.

Even these most urban of settlements appear almost to be lordly properties. Eresburg appears almost as a royal villa, and so it is treated by some historians drawing up lists of Charlemagne's royal estates. Amöneburg was in the hands of the brothers Dettic and Deorulf when Boniface arrived. Würzburg was in the hands of Heden. It does not seem too far wrong to think of Büraburg as Boniface's

estate. Christenberg too is a large site, powerfully enclosed, furnished with a church. Rather than see it as a Frankish military fort, it could well have been the home of a powerful native lord, like Dettic or Heden.

The very process by which these archaeologically investigated sites became waste may suggest they were residences of local magnates. Most of the places Carolingian kings showed military interest in, as revealed by the textual sources, continued to thrive. Rather than see these enclosed sites as deserted fortlets, the reason for their abandonment might be the changing fortunes of a noble family: its extinction or move to another 'seat'. Büraburg soon died out after Boniface moved to Fritzlar.

The smaller sites, especially Hünenkeller, could scarcely fit the bill of a local lord's *curtis* better. Höfe bei Dreihäusen is effectively interpreted that way by German archaeologists, except they try desperately to ascribe it to Charlemagne. Gronauer Altes Schloss would be well interpreted as a local, and perhaps insignificant lord's villa. Tucked discretely away up the Salzode stream it is more likely that it dominated local peasants with the lord's fighting men than that it ever housed a Carolingian army.

The tendency for German archaeologists to attribute all such enclosed sites to kings, dukes, or military and the possibility of an alternative view is seen clearly when one turns to work done in Bavaria. There the same ninth-century enclosed sites are to be found, but the assumed relative political autonomy of Bavaria finds its expression in the social interpretation of these sites: almost all are seen as the residences of local nobles, who exploit the peasants of the neighbouring countryside. Thus Klaus Schwind (1975) attributes seven enclosures to *freie Grundherren*, lords. Two good examples are Castell (fig. 7.9) and Eiringsburg (fig. 7.10). A similar thing is found in the writings of some Saxon archaeologists.

A charter of 816 reveals that a church in Kleinlangheim was in the possession of a near-by estate, Castel. Above the modern village of Castell there is a medieval castle that has obliterated the remains of earlier ramparts. A gateway has been recognised during excavation, dating to the Ottonian period. The Ottonian enclosure may well have superseded an original Carolingian one, the possibility is made more probable by the name of the site. Although the owner is unnamed, Castel was probably the seat of a local noble.

Eiringsburg (Kr. Bad Kissingen, Bayern). On a spur overlooking the Saale river is a 1/2 ha. enclosure, composed of a drystone wall, 2-2.5m. wide, set into loam with larger blocks used in the front and rear face, smaller pieces were used to fill the rubble centre. Around all but the steepest side, on the north, ran a

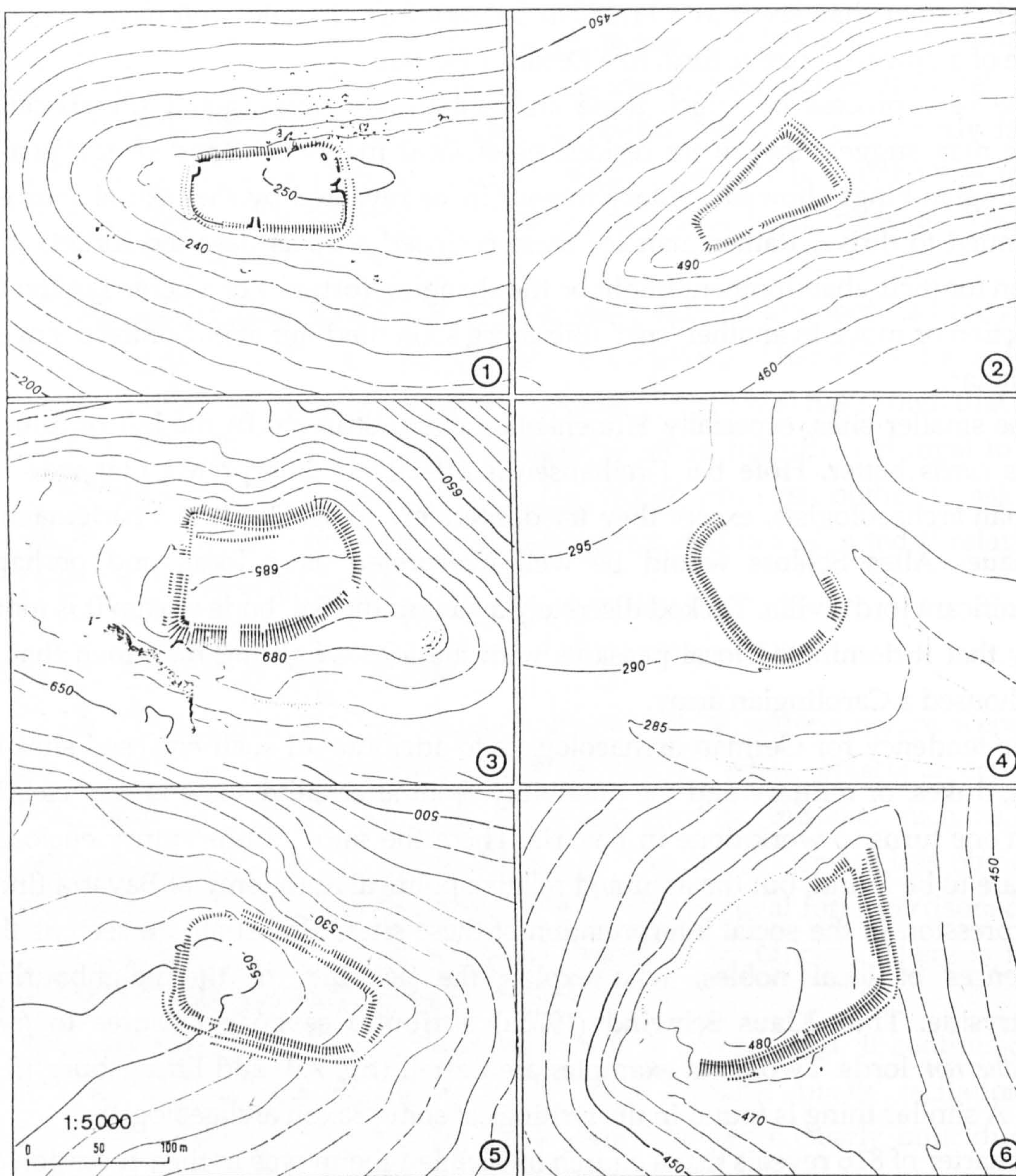


Fig. 7.9 Six small enclosures in Bavaria interpreted by Schwarz as nobles' property: 1. Eiringsburg bei Bad Kissingen, 2. Posserberg bei Oberkups, 3. Hesselberg bei Wassertrüdingen, 4. Burgebrach bei Bamberg, 5. Schloßberg bei Prebitz, 6. Kulch bei Altenbanz.

V-shaped ditch separated from the rampart by a 1.2m. wide berm. Two entrances were composed of inturned wall ends, at the end of which were two post holes that took the gate. The north-western corner produces a very strange stepped effect in plan, which was the site of a simple gap entrance. Finds are consistent with an early ninth century date.

A charter of 822 mentions a local lord, Iring. It is possible that he gave his

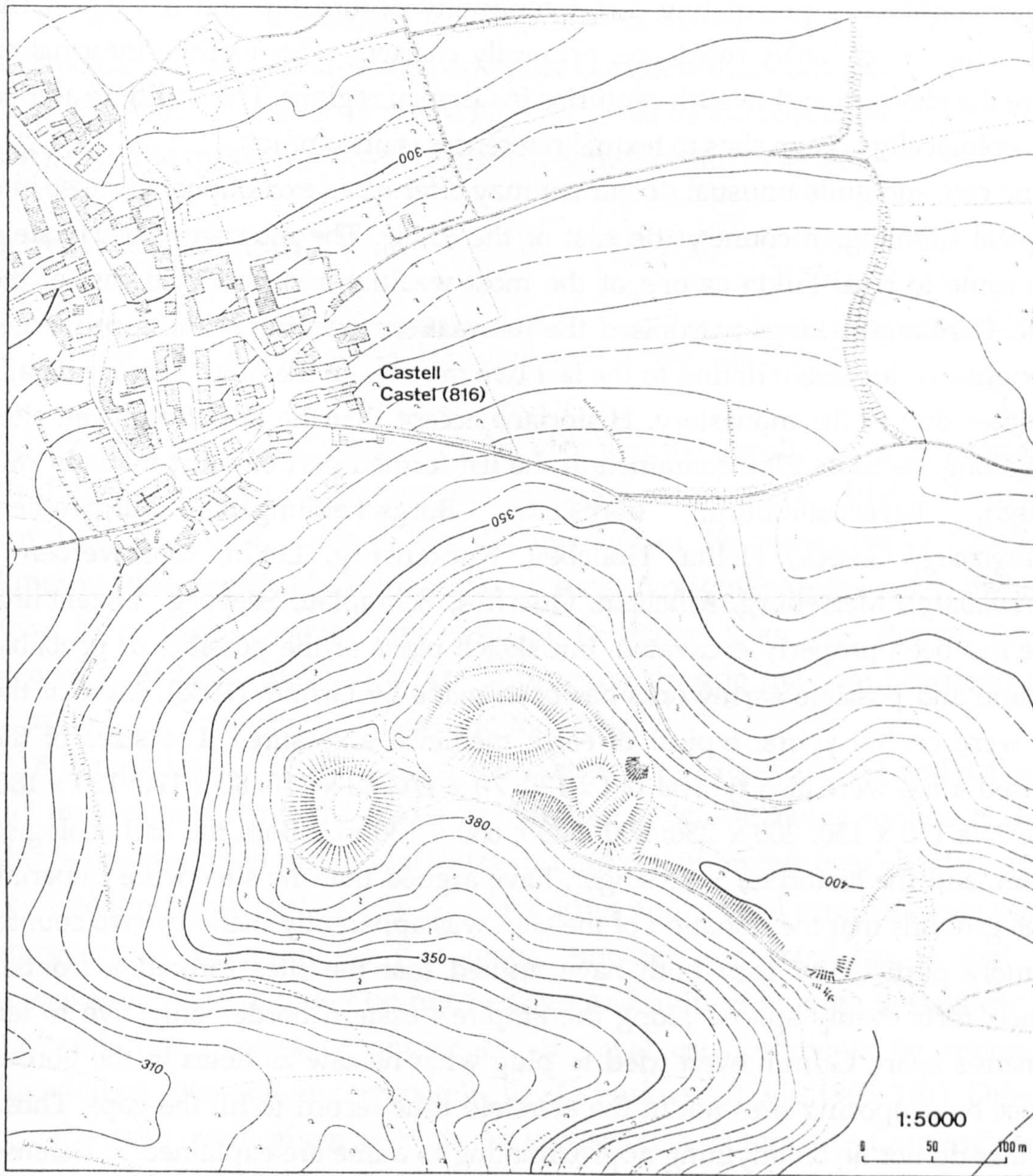


Fig. 7.10 Schloßberg bei Castell. Castell is mentioned in a charter of 816 and thought to refer to a site now obliterated by the medieval castle on the hill.

name to the village and the hill, Eiringsburg. Work in Bavaria has revealed that a large number of Carolingian villas were named after their lordly owners. In this the region contrasts sharply with more 'civilised' parts of the empire.

Bibliography: Schwarz 1975. (see Schwarz 1975 for charter refs.)

Attempts to ascribe more of these Carolingian enclosures to nobles and to see them as residences, as *curtes*, are hampered by the paucity of textual sources and

by the fact that such residences appear to have been shifted to different locations with the rise of what we know as the conventional medieval castle. We are bereft of the possibility of postulating direct continuity of lordship and residence. The textual sources of lordly residence, primarily charters, offer us little information before the eleventh and twelfth centuries in German regions. The ability to ascribe archaeologically known sites to textual references is uncommon.

One rare and quite unusual document may offer us an exceptional glimpse into a typical Carolingian countryside east of the Rhine. The monastery of Hersfeld soon came to rival Fulda as one of the most wealthy and powerful east of the Rhine. Carolingian kings patronised the foundation, granting it many properties and rights. A document dating to the last two decades of the ninth century details the tithes due to the monastery. Historians accept that the list of eighteen sites from along the Saale-Elbe contained in the list formed part of a gift made in 780: Allstedt, Beyernaumburg, Bornstedt, Burgscheidungen, Burgwerben, Gerburgoburg, Goseck, Helfta, Holleben, Kuckenburg, Lettin, Markwerben ? (Uuirbiniburg), Merseburg, MÜcheln, Querfurt, Schraplau, Seeburg, Vitzenburg. None has been properly excavated, but sketch plans of the possible or probable situation and possible earthworks has been made by Grimm (1958). Most of the sites were on hill spurs, typical of early medieval enclosures. The sizes of the enclosed areas were also typical (70 x 130; 70 x 170; 80 x 110; 80 x 170; 120 x 180; 120 x 250; 150 x 150; 200 x 250; 200 x 290; 630 x 130 m.). Both the archaeologist, Grimm, and the historian, Schlesinger, have argued that the sites were imperial, on the grounds that the donation of the sites was apparently made by two counts. But more particularly they both have argued that the sites formed a frontier defence: forts evenly spaced along the empire's eastern border only five to ten kilometres apart. Grimm even tried to plug what he saw as holes in the border defence by proposing sites not in the Hersfeld tithe record to fill the gaps. Those sites that do not lie on anything approximating to a line are explained as defense in depth.

And alternative explanation would be that, rather than the usual donation of scattered possessions, the eighteen sites represent an entire region annexed by Carolingian kings; sites possessed by native nobles. The arguments against the organised fort interpretation have already been outlined above. But the wording of the Herzfeld tithe list also speaks against a military interpretation: *urbes que cum viculis suis et omnibus locis*. The wording follows that of any other well-farmed estate. And why the use of the term *urbes*? All official royal texts that recorded military campaigns used *castrum* or *castellum* for the defensive sites built by the army, never *urbs*. Again the word implies populous sites. Again (chapter two) it

could just be that *urbs* seemed, to a German, the appropriate translation of *-burg* which formed part of the name of all the sites.

It may be that a valuable opportunity has been missed to understand a microcosm of Carolingian settlement more complete than most evidence available. It may be that this corner of the Saale river gives us the best chance of calculating the density of major elite estate centres in eighth-century Germany.

Royal Residence in Villas

In 752 the Carolingians added to their own house-lands the fisc of the Merovingian kings whom they supplanted (Thompson 1935, 7).

It is common to find acceptance of the idea that the Carolingians, descendants of the Pippinid family of palace mayors, managed to inherit most of the Merovingian royal fisc. Property which is first documented in the possession of Carolingian kings is more commonly assumed to go back to original Merovingian ownership than Merovingian estates are assumed to derive from late Roman imperial property. At least, that is the case of property outside the core heartland of the Carolingians, between the Meuse and the Moselle.

A comparison of maps of royal villas attesting visits (figs. 7.11 and 4.4) would not immediately strike the reader as revealing a high degree of continuity. In part this is because royal residential villas did fall out of favour over the decades and were continuously replaced by new favourites and several centuries may separate some of the villas on the two maps. Another important fact is that many monasteries were founded on royal estates and ultimately grew to swallow them, although this process of absorption of royal establishments by monasteries founded on their soil, unfortunately, lies in darkness (Ewig 1965, 161). Otherwise many estates were given outright to monasteries. Before the Carolingians had even received official recognition as kings this had happened on a large scale. Étrepagny, Clichy, and Luzarches were given to St. Denis, and Choisy-au-Bac and Berny-Rivière to St. Médard. Péronne and Lagny-sur-Marne were similarly disposed while abbeys grew up on the soil of Chelles, Crecy-en-Ponthieu, and Nanteuil-sur-Marne. The same may be postulated for Baizieux and Valenciennes.

These estates were not then 'lost' to the Carolingian kings, for during the reigns of Charlemagne and Louis the Pious at least twenty of the most important abbeys were controlled by members of the family or by rewarded members of court. They thus remained within Carolingian patronage.

Carolingian kings often resided at these great monasteries. The first *palatium* explicitly mentioned being built at a monastery for the king's use was by Abbot

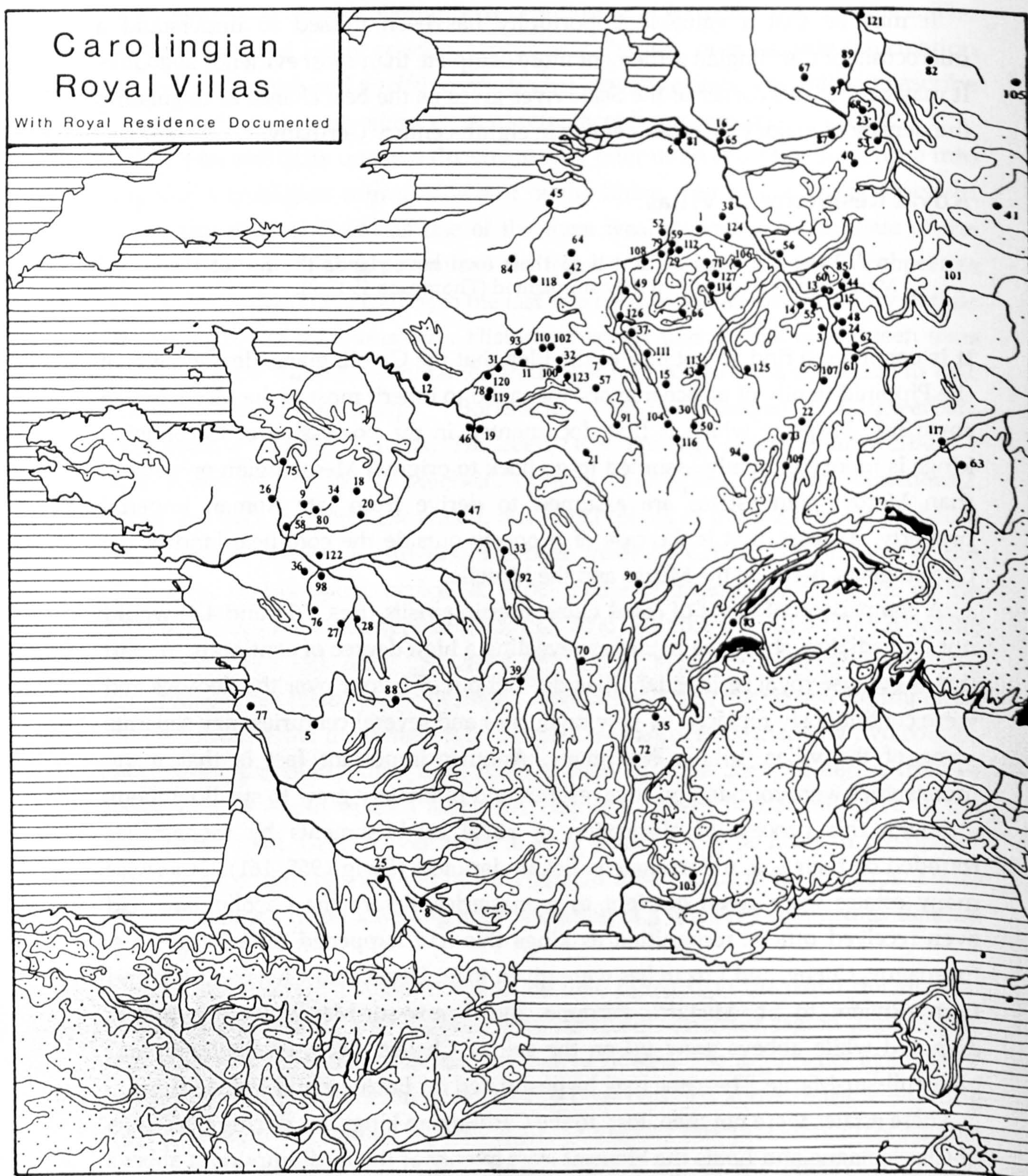


Fig. 7.11 Royal Carolingian villas with attested visits. Table 7.1 provides a key of names to the numbered sites and the medieval source of evidence for residence. The sites in capitals are those discussed under a separate heading within the text of the thesis.

Table 7.1 Royal Carolingian Villas with Attested Visits

| Modern Name | Latin Name (medieval source) |
|--|--|
| 1. AACHEN (Rheinland-Westfalen) | <i>palatium Aquis grani</i> (RFA a. 765, 68, 88, 89, 94, 5, 6, 7...) |
| 2. Aibling (Rosenheim, Bayern) | <i>Eiplingen</i> (Thompson 98) |
| 3. Albisheim (Donnersbergkreis, Rheinland-Pfalz) | <i>Albufilvilla palatium</i> (Thompson 113) |
| 4. Alisni (Niedersachsen) | (Brühl) |
| 5. Angeac (Charente) | <i>Andiacum palatium</i> (<i>Vita Hludovici</i> c.7) |
| 6. Asselt (Netherlands) | (Brühl) |
| 7. Attigny (Ardennes) | <i>Attiniacus villa</i> (RFA a. 765, 71, 85, 822) |
| 8. Avens (Tarn) | <i>Avintus villa</i> (Martindale 1981) |
| 9. Avesé (Sarthe) | (Brühl) |
| 10. Bardowiek (Bad Kissingen, Niedersachsen) | <i>Hliumi-Bardunwih</i> (Thompson 87) |
| 11. Berny-Rivière (Aisne) | <i>Brinnacus</i> (Thompson 67) |
| 12. Bézu St-Éloi (Eure) | <i>salas/palatium Basiu</i> (Brühl) |
| 13. Bibrich (Rhein Lahn kr., Hessen) | <i>Biburg</i> (Thompson 165) |
| 14. Bingen am Rhein (Mainz, Rheinland Pfalz) | <i>Bingia</i> (Thompson 165) |
| 15. Blanzée (Meuse) | <i>Blanciaco palatium</i> (Thompson 74) |
| 16. Bocholt (Borken, Rheinland Westfalen) | <i>Bohholt</i> (Thompson 119) |
| 17. BODMAN (Konstanz, Baden-Württemberg) | <i>Bodoma palatium</i> (Thompson 119) |
| 18. Boëssé-le-Sec (Sarthe) | (Brühl) |
| 19. Bonneuil (Val-de-Marne) | <i>Bonogilus palatium</i> (Thompson 164) |
| 20. Bonneveau (Loir-et-Cher) | (Brühl) |
| 21. Brienne-le-Château (Aube) | <i>Breona</i> (Thompson 120) |
| 22. Brumath (Bas-Rhin) | <i>Brocmagad</i> (Thompson 120) |
| 23. Brunsburg | <i>Brunisburg</i> (Thompson 120) |
| 24. Burstadt (Bergstraße, Hessen) | <i>Bisestat</i> (Thompson 118) |
| 25. Castelferrus (Tarn-et-Garonne) | <i>Ferrucius villa</i> (Martindale 1981) |
| 26. Chambellay (Maine-et-Loire) | (Brühl) |
| 27. Chassenuel-du-Poitou (Vienne) | <i>Cassinogilum villa/palatium</i> (RFA a. 777) |
| 28. Chauppes (Vienne) | <i>Caduppa villa</i> |
| 29. CHEVREMONT (Belgium) | <i>Keversmunt</i> (Thompson 68) |
| 30. Commercy (Meuse) | <i>Commarchiaum</i> (Thompson 123) |
| 31. Compiègne (Oise) | <i>Conpendio villa</i> (RFA a. 757, 79, 816, 17, 23, 24, 27) |
| 32. Corbeny (Aisne) | <i>Corbunacum villa</i> (RFA a. 771) |
| 33. Cosne s. Loire (Nièvre) | (Brühl) |
| 34. Coulaines (Sarthe) | (Brühl) |
| 35. Cremieu (Isère) | <i>Stramiacum palatium</i> (Thompson 83) |
| 36. DOUÉ-LA-FONTAINE (Maine-et-Loire) | <i>Teodadum villa</i> (RFA a. 814; <i>V Hludovici</i> c. 7) |
| 37. Douzy (Ardennes) | <i>Dotciacum villa</i> (RFA a. 777) |
| 38. Düren (Rheinland-Westfalen) | <i>Duria villa</i> (RFA a. 761, 69, 75, 79) |
| 39. Ébreuil (Allier) | <i>Eurogilum palatium</i> (<i>V Hludovici</i> c.7) |
| 40. Eresburg | <i>castrum</i> |
| 41. Erfurt (GDR) | (Brühl) |
| 42. Estinnes (Belgium) | <i>Liptinae</i> (Thompson 69) |
| 43. Florange (Moselle) | <i>Florikingasvilla</i> (Thompson 128) |
| 44. FRANKFURT AM MAIN (Hessen) | <i>Franconofurd palatium</i> (RFA a. 794, 815, 22, 23) |
| 45. Gent (Belgium) | <i>Gandavum</i> (Thompson 129) |
| 46. Gentilly (Val-de-Marne) | <i>Gentiliacum villa</i> (RFA a. 762, 66, 67) |
| 47. Germiny-des-Prés (Loiret) | <i>Gentiliacum palatium</i> (Thompson 163) |
| 48. Gernsheim (Groß-Gerau, Hessen) | <i>Gerunesheim</i> (Thompson 130) |
| 49. Godinne | <i>Goddinga villa palatium</i> (Thompson 130) |
| 50. Gondreville (Meurthe-et-Moselle) | <i>Gundulfi villa palatium</i> (Thompson 131) |
| 51. Hadeln (Cuxhaven, Niedersachsen) | <i>Hauloha</i> (Thompson 86) |
| 52. Herstal (Belgium) | <i>Haristallium villa</i> (RFA a. 770, 1, 2, 6, 8, 83, 818) |
| 53. Herstelle (Höxter, Rhein.-Westfalen) | (Brühl) |
| 54. Hollenstedt (Harburg, Niedersachsen) | <i>Holdunsteti</i> (Thompson 87) |
| 55. INGELHEIM (Mainz, Rheinland-Pfalz) | <i>Ingilenhaim villa</i> (RFA a. 774, 87, 88, 817, 18, 26) |
| 56. Isenburg (Neuwied Rhein, Rheinland-Pfalz) | <i>Isemburgus palatium</i> (Thompson 134) |
| 57. Jouy-lès-Reims (Marne) | (Brühl) |
| 58. Juvardeil (Maine-et-Loire) | (Brühl) |
| 59. Jupille (Belgium) | <i>Iopila</i> (RFA a. 759) |
| 60. Kostheim (Wiesbaden, Hessen) | <i>Copsistaino palatium</i> (Thompson 165) |
| 61. Ladenburg (Heidelberg, Baden-Württemberg) | <i>Lobotenburc</i> (Thompson 166) |
| 62. Lampertheim (Bergstraße, Hessen) | <i>Langbardheim</i> (Thompson 166) |
| 63. Lechfeld | (Thompson 136) |
| 64. Lens (Orne) | <i>Lens palatium</i> (Thompson 136) |
| 65. Lippenham | (Brühl) |
| 66. Longlier (Meuse) | <i>Longlar villa</i> (RFA a. 759, 763) |

| | |
|---|---|
| 67. Lubbecke (Minden-Lübbecke, Rheinland-West.) | <i>Leubice</i> (Thompson 87) |
| 68. Lügde (Lippe, Rhein.-Westfalen) | <i>Liuhidi villa</i> (RFA a.784) |
| 69. Lüne (Lüneburg, Niedersachsen) | <i>Hliuni-Bardunwih</i> (Thompson 87) |
| 70. Mailly (Sàone-et-Loire) | <i>Meltiaco villa publica</i> (Cont. 42) |
| 71. Manderfeld (Belgium) | <i>Manderfelt palatium</i> (Thompson 139) |
| 72. Mantaille (Drôme) | (Brühl) |
| 73. Marlenheim (Bas-Rhin) | <i>Marilegium</i> (Thompson 139) |
| 74. Mattighofen (Austria) | <i>Matachove</i> (Thompson 99) |
| 75. Mayenne (Mayenne) | (Brühl) |
| 76. Moncontour (Vienne) | (Brühl) |
| 77. Mornac (Charente-Maritime) | <i>Criniaco villa</i> (Martindale 1981) |
| 78. Nanteuil-le-Haudouin (Oise) | <i>Nantogilum palatium</i> (Thompson 142) |
| 79. Neufchâteau (Belgium) | <i>Novo castro</i> (Thompson 143) |
| 80. Noyen-sur-Sarthe (Sarthe) | (Brühl) |
| 81. Nijmegen (Belgium) | <i>villa Niumaga</i> (RFA a. 776, 806, 08, 17, 21, 25, 27) |
| 82. Ohrum (Wolfenbüttel, Niedersachsen) | <i>Orhaim</i> (Thompson 144) |
| 83. Orbe (Switzerland) | <i>Urbe</i> (Thompson 84) |
| 84. Orville (Pas-de-Calais) | <i>Audriaca villa palatium</i> (Thompson 166) |
| 85. Ost Rosbach (Wetteraukreis, Hessen) | (Brühl) |
| 86. Ostermiething (Austria) | (Brühl) |
| 87. PADERBORN (Hessen) | <i>Paderbrunnen</i> (RFA a. 777, 83, 99, 815) |
| 88. Le Palais-sur-Vienne (Haute Vienne) | <i>Jogundiaco palatium</i> (Martindale 1981) |
| 89. Petershagen (Minden, Rhein.-Westfalen) | (Brühl) |
| 90. Pontailler-sur-Sàone (Côte-d'Or) | <i>Pontilliacum palatium</i> (Thompson 82) |
| 91. Ponthion (Marne) | <i>Pontico palatium</i> (Thompson 145) |
| 92. Pouilly-sur-Loire (Nièvre) | (Brühl) |
| 93. Quierzy (Aisne) | <i>Carisiacum villa</i> (RFA a.753, 60,1,4, 74, 5, 81, 804, 20) |
| 94. Rambervillers (Vosges) | <i>Rampertvilla</i> (Thompson 145) |
| 95. Ranshofen (Austria) | <i>Rantesdorf</i> (Thompson 89) |
| 96. Ratisbonne | (Brühl) |
| 97. Rehme (Minden-Lübbecke, Rhein.-Westfalen) | <i>Rimie</i> (Thompson 146) |
| 98. Rest | <i>Restis</i> (Thompson 82) |
| 99. Roding (Cham, Bayern) | <i>Rotachim</i> (Thompson 101) |
| 100. Roucy (Aisne) | (Brühl) |
| 101. SALZ (Rhön-Grabfeld, Bayern) | <i>Salz villa</i> (RFA a. 790, 803, 26) |
| 102. SAMOUSSY (Aisne) | <i>Salmontiagum villa</i> (RFA a. 766, 771) |
| 103. Sault (Vaucluse) | (Brühl) |
| 104. Savonnières-en-Woëvre (Meuse) | <i>Saponarius palatium</i> (Thompson 148) |
| 105. Schöningen (Helmstedt, Niedersachsen) | <i>Scahingi</i> (Thompson 89) |
| 106. Schüller (Daun, Rheinland-Pfalz) | <i>Sconilare palarium</i> (Thompson 149) |
| 107. Schweigen (Südliche Weinstraße, Rheinland-Pfalz) | <i>Suega</i> (Thompson 151) |
| 108. Seilles (Belgium) | <i>Silli</i> (RFA a. 806) (Thompson 149) |
| 109. Sélestat (Bas-Rhin) | <i>Scladdistat villa</i> (RFA a.775) |
| 110. Servais (Aisne) | <i>Silviacus palatium</i> (Thompson 150) |
| 111. Stenay (Meuse) | <i>Satanacum palatium</i> (Thompson 82) |
| 112. Theux (Belgium) | <i>Tectis palatium</i> (Thompson 152) |
| 113. Thionville (Meuse) | <i>Teodonevilla villa</i> (RFA a. 773, 82, 805, 06, 21) |
| 114. Thommen (Belgium) | <i>Tumbis palatium</i> (Thompson 83) |
| 115. Trebur (Groß-Gerau, Hessen) | <i>Triburium palatium</i> (Thompson 168) |
| 116. Tusey (Meuse) | <i>Tusiicum palatium</i> (Thompson 84) |
| 117. Ulm (Baden-Württemberg) | <i>Ulma palatium</i> (Thompson 90) |
| 118. Valenciennes (Nord) | <i>Valentianae palatium</i> (Thompson 155) |
| 119. Ver-sur-Launette (Oise) | <i>Vernus palatium</i> (Thompson 156) |
| 120. Verberie (Oise) | <i>Vermeria</i> (Thompson 71) |
| 121. Verden (Niedersachsen) | <i>Verdia</i> (Thompson 90) |
| 122. Vernantes (Maine-et-Loire) | (Brühl) |
| 123. Verzenay (Loire) | <i>Virciniacum villa</i> (RFA a. 779) |
| 124. Vlatten (Düren, Rheinland-Pfalz) | (Brühl) |
| 125. Volkingen (Saarbrücken, Saar) | <i>Fulcolingae</i> (Thompson 129) |
| 126. Warcq | (Brühl) |
| 127. Weims (Belgium) | (Brühl) |
| 128. ZÜRICH (Switzerland) | Erdman 1979 |

Fardulf (793-806) at St. Denis, which, because of the monastery's importance and the frequency with which it received royal visitors, may well have been the first such palace (Brühl 1968, 29). During the course of the ninth century explicit reference is made to such royal palaces at St. Alban at Mainz, St. Arnulf at Metz, St. Emmeran at Regensburg, St. Germain at Auxerre, St. Jean and Notre-Dame at Laon, St. Loup at Troyes, St. Médard at Soissons, and St. Remi at Reims. The supposition that comparable *palatia* existed at many other great abbeys and cathedrals is inescapable. We have, unfortunately, no examples of such *Klosterpfalzen*, although one such building is probably represented in the St. Gallen plan (see fig. 7.12).

In Brühl's research into the royal right to 'hospitality' he discovered that dependence on monastic houses is only first apparent in the reign of Louis the Pious, represented in the attestation of six per cent of royal charters while visiting a monastery. This dependence increased in the reign of Charles the Bald who attested twenty per cent of his charters in monasteries. For the rest of the time Carolingians appear to have lived in their own palaces. Episcopal centres did not play anything like the role of the great abbeys. Archbishop Leidard of Lyon recorded his construction of a *domus cum solaris* for Charlemagne's use (Brühl 1968, 25), but other evidence for such royal apartments at cathedrals is rare.

Given that Carolingian sejours were only infrequent at the great monastic houses, that such were chosen in preference to episcopal centres when they were suburban monasteries, and that a royal visit to episcopal centres, even when they occurred, lasted only for days, Brühl (1968, 25) concludes that although some Carolingian urban palaces must have continued from the Merovingian period many must have disappeared. Only at Worms, Poitiers, and Regensburg are royal *palatia* explicitly recorded, although Gauert (1965b, 313) assumes they existed at Reims, Metz, Orleans, Noyon, Soissons, Speyer, and Mainz as well during the reign of Charlemagne. Although a search has been made for the palace at Worms its site has not been located. This raises more problems for Brühl's hypothesis that Merovingian urban palaces probably underlie the later *palais de justice*, which was discussed in chapter five, for their fate in the Carolingian period is in need of fuller explanation to strengthen his hypothesis.

The rural villas of the Carolingians, without any doubt, were the most important centres of residence. Brühl (1977, 424) dismantles the inconsistent use of evidence in the argument that the *atavisme germanique* and their inherent 'love of nature' prevented the Carolingians from residing in towns. Such a nonsense needs no elaborate refutation, but is mentioned here because such a mythical explanation reveals just how strikingly common royal Carolingian residence on

rural villas was. Of course Frankish kings did enjoy nature for the opportunity it gave them to kill wild animals.

Perhaps the only specialised residential villas were those that might be considered as hunting lodges. Gauert (1965b, 316) considers Champ-le-Duc in the Vosges as one such site belonging to Charlemagne, although the documentary evidence is not clear enough to tell us exactly what kind of site it was. Remiremont was the regular hunting retreat in the Vosges. But it is impossible to decide whether the kings stayed at the monastery there or whether they maintained a separate establishment. Given what was said above about monastic houses swallowing royal villas, it might seem probable that the Carolingians were accommodated by the monastery. On the other hand, Remiremont was visited more often than most monasteries and hunting, a very worldly pursuit, is the only royal activity recorded at Remiremont, so the continued existence of a royal villa should not be ruled out. It has been suggested that Nijmegen owed its origin as a hunting lodge, although it was one of Charlemagne's three great new palace projects and might therefore have been supposed from the beginning to have had a fuller role to play. The Ardennes surprisingly did not spawn a purpose-built hunting villa that we know of, but it is possible that during long stays at Herstal, Aachen, or Thionville the Carolingians visited hunting lodges in the Ardennes of which we have no record. Hunting, however, was something which could be practiced in the neighbourhood of almost any villa. One is struck by the Merovingian King Chilperic's ability to hunt at Chelles in the Paris environs. When Louis the Pious, an extremely keen huntsman, set off from Ingelheim, we need not even envisage a journey to the near-by Eifel hills, for the *Poem to Louis*¹ clearly reveals a game park (fig. 6.5), which must have been artificially stocked, at which Louis could satisfy his bloodlust within sight of his palace. It has been argued that gameparks were a common feature of Carolingian royal villas. *Brogilos* are mentioned in the *Capitulaire de villis* (c.46) and in other capitularies; they are individually mentioned at Attigny, Frankfurt, Compiègne, and several other villas in the late Carolingian period. These were apparently woodland gameparks; such an interpretation would make explicable the references to fences which must be repaired and the naming of people to manage them.

Medieval estates were more than just homes designed for the comfortable living of the wealthy. 'There can be no doubt that he [Dhondt] was correct in asserting that land formed the basis upon which Carolingian power rested,' writes Jane Martindale (1983, 173) as one of her four conclusions about the dissolution of the Carolingian fisc. Since James Thompson (1935) wrote his book

¹ Lines 2366-2437.

by that title and Dhondt (1948) concurred with the conclusions – although he argued the case with more particularist studies – it has been a cornerstone of understanding Carolingian politics that royal power depended on the number of estates owned and exploited. Martindale, however, takes issue with *how* the Carolingian fisc came to be ‘dissolved’. She argues that the cause was not the recklessness and wantonness of Charlemagne’s successors.

In the course of her arguments, Martindale assembles evidence to show how Carolingian monarchs recognised the importance of their estates for their agricultural production. Those invaluable sources, the *Capitulare de villis* and the *Brevium Exempla*, have long been known and extensively studied. They reveal clearly how much care Carolingian kings took in assuring good management of their estates. Wolfgang Metz (1960) devoted an entire book to the subject of royal Carolingian estate management and found that Charlemagne’s successors were no less diligent. Charles the Bald’s last capitulary, issued even as he prepared a military campaign to Italy (AD 877), even sought to prevent unauthorised persons from enjoying hospitality on either his or his wife’s property.¹

First and foremost, estates had to feed the king and his household. This is made clear by a passage from the Astronomer’s *Life of Emperor Louis* (1.7):

The king showed proof of his foresight and disclosed the disposition of his mercy. He ordained that he would establish winter quarters in four places, namely, the palaces of Doué, Chasseneuil, Angeac, and Ebreuil, so that after a lapse of three years each place would support him during the winter in the fourth year only. Those places would then offer sufficient provision for the royal household when it came back for the fourth year.

The Carolingians, just as their Merovingian predecessors had, moved from estate to estate, consuming the produce of each. The Lorsch annals record, under the year 800, ‘*et circa quadragesime tempus circumivit villas suas.*’¹

An important aspect in creating major residential centres relates to the transportation of agricultural surplus from other estates. Great men did not have to visit each and every estate in turn, for the most important centres would be supplied by many others. Arguably, the most important specialisation among estates was a simple division between those which were purely agricultural and those which further accommodated their owners.

One form of specialisation was the growing of vines. Northern based churches and aristocracy often seem to have made provision to obtain southern villas in order to get hold of Mediterranean products. Patriotic southern French historians, such as Ruche, have graphically illustrated the situation on maps with lines drawn from large monastic houses in the north of France to their southern

¹ Martindale 1983, 163-7.

properties. The figures are then likened to an octopus with tentacles wrapped around the south.

The necessity for king or nobles to wander, munching their way through stocks, progressively lessened through the Carolingian period. The growing use of coin and increase in number and importance of markets meant that agricultural surplus and even social dependency could be converted increasingly easily into cash. Regular alteration as in the case of Louis in Aquitaine cannot be found elsewhere. In part the reason was political. When analysing the places at which Charlemagne resided, Gauert (1965b) notes that the most important factor in a majority of cases was the political situation. Charlemagne moved about as he was compelled. Paderborn was often sought out but usually as the result of campaigns against the Saxons. All, however, was not random. Ewig (1965) notes that royal activity was dependent on the seasons. Thus autumn and winter was spent in general inactivity other than hunting, while spring and summer were used for extensive travelling and campaigning. During this warmer half of the year Carolingian kings could not count on staying in one spot for any great length of time, but as it was the period of easiest travel, it was also in this season that general assemblies were held. By contrast, the relative immobility of the autumn and winter meant that selected villas would be destined to accommodate the king and his retinue for a long period, during which the next years' action could be planned. In effect winter palaces were created: villas that could cope with the excessive demands of an otherwise itinerant court. This is exactly what the 'Astronomer' describes of Louis's actions, as king of Aquitaine.

Anachronistically, but only just, some historians refer to Aachen as a capital. The Carolingian court was peripatetic, but from 794 onwards there was probably not a year that Charlemagne did not visit Aachen, indeed he probably spent all but four of his last twenty Christmases there. From Gauert's (1965) itinerary map, Charlemagne visited Aachen more than twice as many times as the next most frequented villa, Herstal and nearly four times oftener than the next two, Quierzy and Thionville. Under his successor, Louis the Pious, Aachen was even more frequently visited, three times more often than the next five favourites, Thionville, Compiègne, Ingelheim, Frankfurt, and Nijmegen. Aachen had grown in importance during Charlemagne's reign, clearly replacing Herstal which had been the most frequented villa during the first decade of Charlemagne's reign. Although Aachen did not function as a capital, for the executive and even administrative bodies of government were not static and followed the emperor around his kingdoms, it was a preferred site and contemporaries could count on the emperor's return to Aachen, generally within the year.

Martindale (1983, 174) recognises that villas were more than just sources of economic power, for they were of 'central importance in the conduct of early medieval government and administration'. This was her final concluding point. Important residential villas, such as the four winter palaces of Louis as king of Aquitaine, would dwindle when no king came to visit while on his rounds. Before discussing 'dissolution' we must be aware that we are watching a kaleidoscope; some royal villas dwindle in importance or are even alienated while new villas grow in status to take their place. This constant shifting reflects changes in the political situation. It might also be added that the reflection of a more prosaic phenomenon is to be found here: dilapidation. As newer and bigger palaces were thought necessary, it was probably more often convenient to build afresh rather than extend older buildings or tear them down only to start again. Thus it was that the series of important Merovingian villas, with which this chapter began, when donated to monasteries by the early Carolingians, had in many cases first seen royal residence over a century earlier.

Old dilapidated palaces could not suffice for royal needs, for they were more than just the sources of economic power. They were symbols of their lordship and they were often in view of the most powerful political figures of the day. The most important of the rural villas of the Carolingian kings were where much of government and administration was conducted. It was here that charters were issued by kings, not in cities, not by a central chancery and not by the recipients themselves. Although Charles the Bald appears to have depended more heavily than any of his predecessors or even contemporary branches of the family on monastic houses, yet then only twenty per cent of the charters he issued were while resident at monastic establishments. Almost all the remainder were issued while he was on his own estates.

It was not just in the promulgations of the kings that villas played such a central role, for it was here that the king met his friends and his enemies for discussion and the formulation of policies. Assemblies were held at Aachen, Ingelheim, Paderborn, Frankfurt, Thionville, Quierzy, Düren, Nijmegen, Gentilly, Compiègne, and Attigny, just to name the instances recorded in the *RFA*. Indeed, many more must be added to these, particularly those at which the *RFA* record the celebration of Easter or Christmas, for at such important festivals there were often large numbers of nobles present and the occasion was used for lesser assemblies. Otherwise envoys were regularly received at any villa at which the king happened to be resident. This included even the lesser important villas, so that Charlemagne met the nobles of his recently deceased brother at Corbeny, envoys from Byzantium came to Salz, and the duke of Spoleto came to Verzenay.

Saxon nobles, including Widukind, were baptised at Attigny and King Harald of the Danes at Ingelheim. The latter incident is described in some detail in the *Poem to Louis* which reveals that entertainment of foreign dignitaries included the normal royal pastimes of feasting and hunting.

Aachen may have impressed contemporaries by its cosmopolitan nature more than by its architecture. Aachen was frequently the venue for general assemblies and the celebration of the major Christian festivals of Easter and Christmas. In the *RFA*, under AD 812, we read that Charlemagne sent his grandson to Italy after 'the general assembly, held in the usual manner at Aachen'. For the year 825 the *RFA* state of Louis the Pious that he 'celebrated the holy feast at Easter as usual at Aachen'. During the thirty years previous Aachen had certainly become the preferred location for such gatherings. It was also the preferred site for the reception of foreign embassies. At Aachen, ambassadors could be found from the Danes or Slavs, from the governor of Sicily or Saragossa, the patriarch of Jerusalem, and from the Byzantine emperor or the Caliph of Bagdad. It was also where Alcuin hoped Charlemagne would receive the pope.

The Aula Regia. The machinery of government and the process of forming consensus, mainly through assemblies, demanded of royal residences something that the majority of ordinary nobles could do without: very large halls. While it is a truism that medieval royal authority was simply normal lordship writ large, in certain areas the writing was very large indeed. The convenient sheltering of large gatherings of the most powerful men in the kingdom was one such area.

Figure 7.12 compares most of the archaeologically known royal Carolingian halls, with a few others thrown in for comparison. That at Aachen was without rival. They reveal a variety of constructional forms in the way the roof was carried. Some were undivided halls, some had a single row of columns supporting the roof ridge, and others were double-columned almost certainly supporting a purlined-roof. Whether or not any of the halls were basilican, that is with walls above the two central columns, is unknown. Such a reconstruction gives a Roman air to the building, but a good argument against it is that the interior supports were probably of timber on stone bases. This almost certainly speaks against a basilican form. Exterior walls were surely load bearing, and, in all cases reviewed in the last chapter, were of stone held together with mortar. The *Brevium Exempla*, which records some modest royal estates, allows us to infer that the residential manor was regularly built of stone, even when all the other villa buildings were of wood.

One rather special characteristic of some of these halls was the addition of a

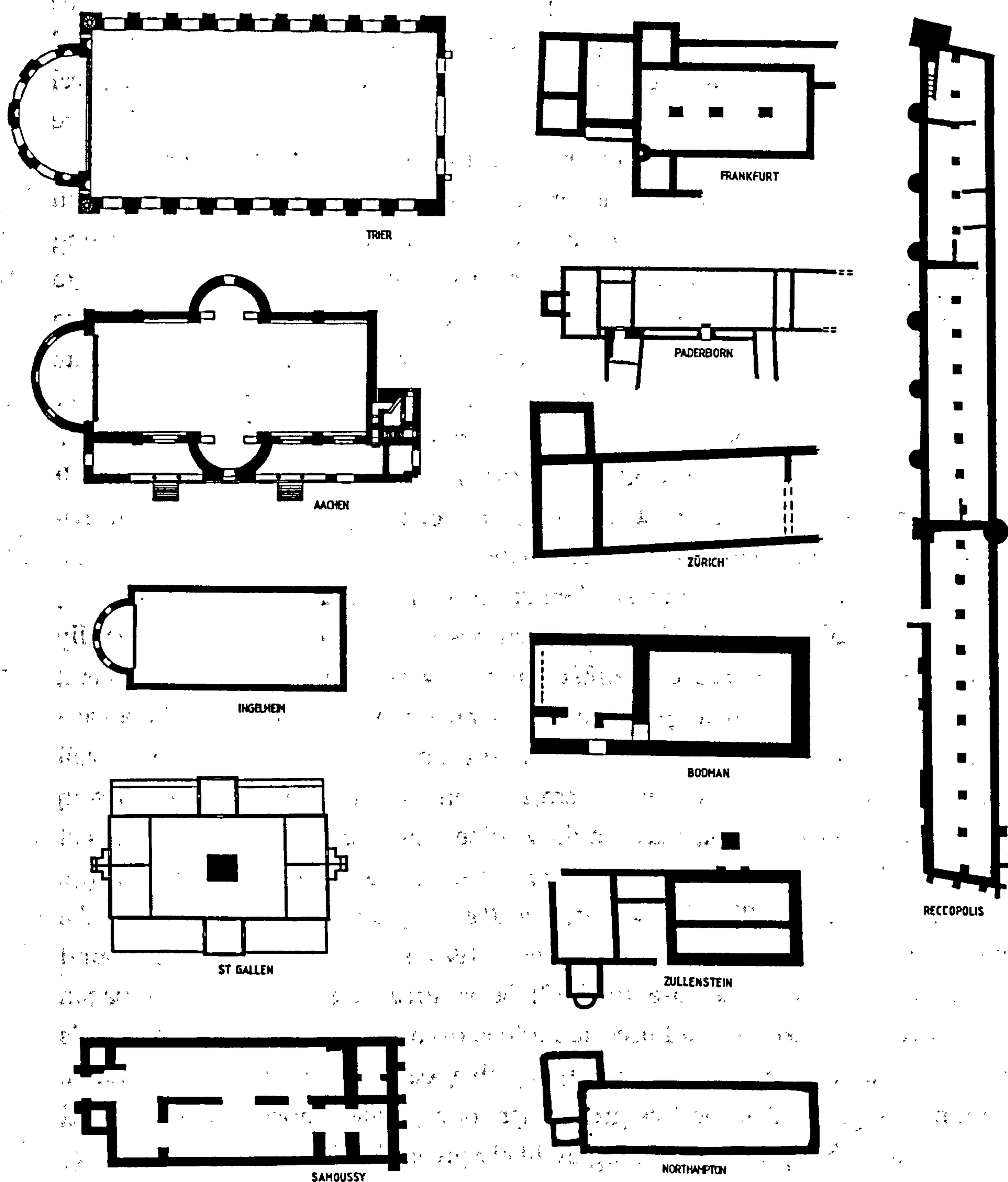


Fig. 7.12 Comparative plan of early medieval royal halls.

semi-circular or rectangular apse. This, one must assume, was where the king would have been enthroned or stood to address the assembled. In the last chapter it was suggested that the secular-ecclesiastical oppositions were taken to the extreme in the use of such elevated niches at Aachen. In the great hall the king

could face inwards to those assembled under the roof, from the Minster first floor western niche the bishop (or perhaps the king) could face outwards to the assembled in the open. The annex buildings on either side of the Minster had niches and assembly halls, one with aisles and thus with more ecclesiastical connotations, one without aisles with a more secular flavour. I suggested that one was for the king's sacred functions, the other for the bishop's secular functions.

A throne baldachin was found at Paderborn outside the hall in the open courtyard. Again there seems to have been arrangements made for addressing assemblies either inside or outside. Such distinctions were presumably due to more than just the vagaries of weather or the size of the gathering, but were related to the types of audience and the degree to which it was held to participate in the meeting or passively receive a message. No more clear example of the latter was the distribution of largesse. The *solarium ad dispensandum* above the stone gateway at Asnapius mentioned in the *Brevium Exempla* is quite well known. It was presumably from the first storey balcony of the gate-tower at Aachen that such alms were given by Charlemagne to the poor.

The most important meetings, whether assemblies or the reception of envoys, would have taken place in the august settings of the reception halls, especially decorated for the purpose. Painted plaster was found at Paderborn and Ingelheim, and we must suppose that it was much more commonly used than just the major palaces, given that most of our knowledge of Carolingian wall paintings come from some of the smallest and most isolated of churches in present-day Europe, particularly in the Alpine region, or the deepest recesses of crypts in larger churches, where the only vestiges of Carolingian architecture remains. Ermold the Black's *Poem to Louis the Pious* (lines 2126-63) describes the paintings of the *domus regia* which Lammers (1973) situates hypothetically around the main walls and in the apse (fig. 7.13). Seven great rulers of the ancient pagan world were depicted. Ermold uses quotations from Orosius, which was either his own invention or, and this is more probable, each scene bore the quotation. Lammers suggests that the five great rulers of the new, Christian world would have been in the apse, which is highly likely given that it was presumably here that the emperors sat. These five were Constantine, Theodosius, Charlemagne himself, his father Pippin, and his grandfather Charles Martel.

The appearance of Charlemagne together with Constantine is hardly surprising; it was a common enough theme. In 799 in a poem celebrating the meeting of Charlemagne and Leo III, Aachen was described as a 'second Rome'. The epithet *Roma nova* was very apt for it looked ideologically to both Rome and Constantinople, which was Constantine's *Roma nova*. As *Roma nova*, Aachen could

¹ *Karolus Magnus et Leo papa* (MGH Poetae 1, ed. Dümmler, 377ff.).

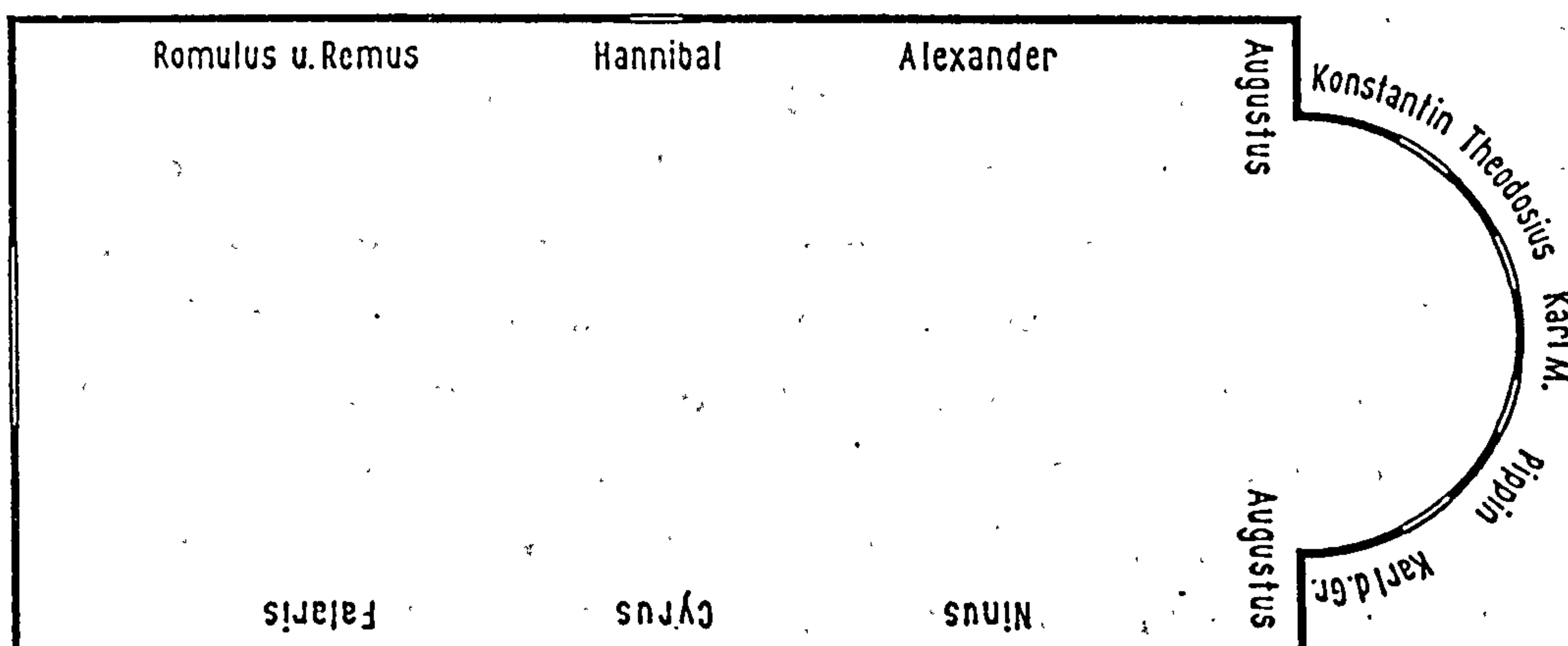


Fig. 7.13 Hypothetical arrangement of wall scenes in the aula regia at Ingelheim, based on a reading of the Poem to Louis the Pious by Ermold the Black (after Lammers).

be seen as the western equivalent of Constantinople. No small amount of effort has been expended on the question of Charlemagne's rise to imperial status and the 'constitutional' position it created *vis-à-vis* the Byzantine empire. In terms of the 'Donation of Constantine' the pope was given jurisdiction over the western half of the Roman empire, which in its secular aspects was in turn conferred upon the Carolingian emperor. Thus Aachen became the secular equivalent of Rome.

As Krautheimer (1941) first pointed out, the great fourth-century basilicas of Rome with aisled naves, continuous transepts and single apses, lay dormant nearly half a millennium as a source of architectural inspiration until the beginning of the ninth century. Suddenly the great churches of St. Peter and St. Paul were seized as models for new constructions in Rome and north of the Alps, particularly at Fulda and Einhard's churches at Seligenstadt. This has been interpreted and generally accepted as a conscious attempt to revive the grandeur of the first great Christian emperor. Charlemagne, apart from being referred to as King David, was also called the 'New Constantine' by scribes of the papal chancellery and other contemporaries. He was depicted in a mosaic in the triclinium at the Lateran palace in Rome on one side of St. Peter with Pope Leo III on the other, while a second group represented Constantine, Christ, and Sylvester. Charlemagne was clearly being represented here as a new Constantine. The famous gate-tower at Lorsch monastery is shown by Krautheimer to owe its inspiration most probably to Constantine's arch in Rome, and it was in the ninth century that the term *arcus triumphalis* was first applied to the arch between the nave and transept, a term which we customarily apply to Roman arches, but which had been very unusual in Antiquity and only found in a similar form,

arcu(s) triumph(is) insign(is), once in Rome – on Constantine's arch.

The borrowings from Rome may have been more important than those from Constantinople. In particular, the application of the name Lateran to some *palatium* at Aachen was remarkable. Krautheimer (1942, 35) argues that a reference in the Moissac chronicles refers to Aachen as a whole as being called Lateran, although the *palatium quod nominavit Lateranis* which he had built 'in Aachen' almost assuredly refers to a structure, not the whole palace complex. In light of this connection, it may be of some importance that the Lateran palace baths contrasted with other episcopal baths which were shared with the urban clergy by being maintained by popes as 'a personal and lavish establishment' (Ward-Perkins 1984, 146). Such a description would equally well fit Aachen.

Statues which ornamented Aachen all have close Roman parallels. I have already mentioned the equestrian statue mirroring Marcus Aurelius, alias *caballus Constantini*, at the Lateran. To this we might add the bronze figure of a she-bear/she-wolf which sat in the porch of Aachen's cathedral. It may have been intended to parallel the Roman *Lupa* maintained, again, at the Lateran palace. The famous pine-cone fountain which sat in the middle of St. Peter's *atrium* in Rome was without doubt, the model for Charlemagne's pine-cone fountain at Aachen (Ward-Perkins 1984, 143).

It would be wrong to see a consistent plan to depict Charlemagne as a new Constantine, for other imperial or royal parallels were gladly accepted. In particular, Theodoric the Great was a Carolingian hero, and it was an equestrian statue of Theodoric that Charlemagne wanted from Ravenna to adorn Aachen, notwithstanding the good possibility that the statue was in fact the emperor Zeno. Such a statue was particularly desirable, for the statue of Marcus Aurelius at the Lateran palace was a famous landmark, although interpreted as *caballus Constantini* by the antiquarians of the Middle Ages. Furthermore, Justinian had an equestrian statue of himself on the Augusteion between the imperial palace and Hagia Sophia (Bandmann 1965, 451). On the strength of this as a possible analogy it is suggested that the Theodoric statue was erected in the Aachen courtyard between the *aula regia* and cathedral.

The possibility that the Carolingians saw Theodoric's palace church in San Vitale instead of Sant'Apollinare is sometimes argued, although this seems unlikely given the Justinian mosaic within. The similarity of the Minster and *atrium* facade with that of the so-called palace of the Exarch in Ravenna has been noted above, although not that its traditional epithet is, interestingly, 'Theodoric's palace'.

A perhaps even less expected possible source of inspiration for Aachen was

Benevento. Lombard dukes built the centrally-planned St. Sofia as part of the palace complex and the Lombards used the term *sacrum palatium* almost a century before the Carolingians first adopted the term to apply to royal villas as well as the court in 794 (Bullough 1965, 166).

'The "sacred palace" was, for some learned Carolingians, comparable to the Temple of Solomon in as much as it prefigured the celestial Jerusalem' writes Riché (1976, 167). The confusion between palace, temple, and church becomes complete in the illustrations of the *Utrecht Psalter*. Furthermore, claims Riché (1976, 168) when quoting Rabanus Maurus, 'If the royal palace was the anticipation of paradise, paradise, where God reigns, and God's servants were very often described as a palace.' Riché puts too much temple into the palace. Just as man in fact creates God in his own image and imagines the reverse to be true, so the image of the sacred palace must have originated in an attempt to visualise where and how God reigned in the heavens. It is for exactly this reason that Riché can find a supposed tradition in Germanic myth. Valhall was no more than a supernal long hall, just as the houses in heaven conceived by Gregory the Great in his *Dialogues* were probably no more than typical urban Roman dwellings, only built of gold. God could not rule heaven from a tent. Sacred connotations, I would argue, are seldom to be found in the architecture of palace buildings, chapels and churches naturally excluded. It was God who borrowed the earthly palace in which to rule, not vice versa.

The symbolic importance of the palace residences is recognisable in literary expression. Einhard, in imitation of Suetonius, enumerated the great architectural undertakings of Charlemagne, which included the building of Aachen, Ingelheim, and Nijmegen palaces.¹ Ermold the Black praised two palaces in Aquitaine built by Louis the Pious, Doué-la-Fontaine² and Angeac³, in addition to Ingelheim. Perhaps even more interesting than the glowing terms Ermold used to describe these villas of his Carolingian master is the way he denigrated the home of Louis' enemy, the Breton Murman, which was surrounded by 'bushes, ditches, and a marsh'.⁴

Sadly, it may well have been the very paucity of classical examples to copy which led to such a minute quantity of literary description of architectural detail being written. Unlike Fortunatus Carolingian poets did not seem to find Sidonius Apollinaris's style imitable, although Ermold the Black's idyllic pastoral setting of Angeac and Doué do strike some familiar chords. As laudable as the sentiments of Hincmar (*De ordine palatii*) are, that 'the royal palace is such because of the rational men who inhabit it, not the insensible walls or masonry'⁵ they do not help the archaeologist.

¹ *Vita Karoli II*. 17.

⁵ Riché 1976, 164.

² *Poem to Louis the Pious* lines 744-753.

³ *Poem to King Pepin* lines 7-16.

⁴ *Poem to Louis the Pious* lines 1346-1351.

There is little that can be said about the possibility of residential quarters within these *aulae*. Most excavators believe the buildings were two-storeyed, in the upper of which we might expect the accommodation to have been. The little chambers sometimes found off the main hall could have served any number of functions; at Paderborn one was perhaps a toilet.

Another peculiar architectural element of these royal villas is worth remarking. At Aachen, Ingelheim, Frankfurt, and Paderborn we find evidence of a *porticus*. At all of these sites the hall facade was fronted by an arcade, and at all of these sites, although the case of Paderborn is not clear, the portico runs to join the church. This raises the question, were they the product of a renaissance or do they represent the unbroken continuity of tradition from the *galerie de façade* of Gallo-Roman villas through Merovingian villas to the early ninth century. Without the excavation of some important royal Merovingian palaces we will probably be unable to say for certain, but two things point to them being a novel arrangement. Firstly, they are only found at the great important palaces, but not at the smaller ones. Thus they appear a rather special elaboration, and one that does not continue to be used. Secondly, the connection with churches may explain their source of inspiration: a sort of cloistered walk.

Summarising the royal Carolingian *aula regia* highlights a problem often glossed over: was the royal hall strictly for ceremonial gatherings or did it incorporate a royal residence as well? The large halls at Aachen and Ingelheim with their apses in which the emperor surely sat enthroned must have been the venue of the documented assemblies at these sites. Paderborn similarly was the setting for four documented general assemblies in Charlemagne's time. There, however, the dimensions of the hall are comparable to the large buildings uncovered at Bodman and Zürich. The latter two are perhaps a century younger than Paderborn, which may account for some of their increase in size, but Bodman received one visit from each of Louis the Pious and Louis the German as documented by charter attestation, otherwise it did not figure in the 'historical' events of their reigns and one cannot imagine them as having been built with the need for an assembly hall in mind. Thus we are faced with the possibility that these large halls were typically large for another reason, such as accommodating the royal household. In that case, fairly large halls at the villas of other nobles would not be unlikely.

Elements of Carolingian Villas

The Hall. Certainly for nobles the hall represented a living area as well as an audience hall. The drawing room of today owes its name to the late

medieval/early modern development of the 'withdrawing' room, a room to which a lord and select guests could retire from the main chamber. Only in the later Middle Ages did 'the chamber' lose its dual function of reception and habitation.

Icelandic sagas reveal revelry, conspiracy, feasting, domestic chores, and sleeping all occurring in the hall. The Anglo-Saxon timber 'palace' of which we now know so much must have functioned similarly.

Anglo-Saxon timber halls are quite distinctive, but comparable long, timber-framed halls existed in German regions (chapter two). The earliest Carolingian manorial halls were probably little different. At Hünenkeller the boat-shaped hall was an impressive 23 metres long. It is possible that the 10-metre long stone hall at Höfe bei Dreihäusen was the main hall, but it seems likely that something larger would have existed, given the remains of what was assuredly an impressive little round church. Halls are otherwise absent from our archaeological record. We can assume, however, that the central residence at Christenberg and Büraburg were not only large, but certainly of stone.

It is only during the Carolingian period that we find the first Anglo-Saxon palace being built in stone to replace a previous timber hall, at Northampton (Williams 1985). But stone-built halls were possibly the norm in Carolingian Gaul. All the evidence that was gathered on Merovingian villas pointed to a *domus* of stone. Certainly all the major palaces of Carolingian kings were stone, and probably their lesser homes too. At Annapes the *sala regalis* was built of stone, was of three rooms with a further eleven rooms for women either above or attached and a cellar below (*Brevium Exempla*, chapter six). At Treola the royal house was likewise of stone, but only had two chambers. One unnamed estate had a house in stone, and a more modest unnamed estate had a house of wood and only one chamber.

In addition to concluding that the typical Carolingian noble's *domus* was built of stone, we can also hypothesise that it was subdivided into several rooms.

The buildings were probably heated by the tried and true method of open hearths. It was only from the Carolingian period that the fireplace and chimney apparently began to be used. Only at Doué is there any evidence for such an arrangement.

Churches and chapels. Aachen was clearly Charlemagne's most important palace, and contemporaries were particularly impressed by its cathedral. Einhard (VK 1.17) singled it out as outstanding among all of Charlemagne's building projects. Notker (*De Gesta Caroli Magni* 1.28) was similarly enthusiastic: 'He

conceived the idea of constructing on his native soil and according to his own plan a cathedral which should be finer than the ancient buildings of the Romans.' The source of inspiration for the cathedral in Aachen has been the theme of many works, some of which is summarised by Bandmann (1965). Some of the most important 'circular' churches that may have been influential include, the 'Octagon' in Antioch, Ss. Segios and Bacchos and Hagia Sophia in Constantinople, the Pantheon in Rome, San Lorenzo in Milan, San Vitale in Ravenna, St. Gereon in Köln, and several mausolea, particularly at Split, Thessalonika, Constantinople, and Rome. The two cannot be easily separated as the circular churches had developed as martyria from mausolea, while secular mausolea were themselves usually converted into churches or chapels in the post-Roman period. In the case of the two imperial mausolea on the south side of St. Peter's in Rome, one was converted into a chapel to St. Andrew and the other, much later, into a chapel dedicated to St. Petronilla in 757 by King Pippin. It has been suggested that this close connection – the chapel became known as *capella Francorum* – influenced the construction of the supposed round church of Pippin at Aachen. However, as a martyrium, Aachen minster was not well suited with its vast collection of relics and its dedication to the Virgin, although it was ultimately to form a 'mausoleum' for Charlemagne. Perhaps a more important connection was that of imperial palaces and round churches. Split was Diocletian's palace, Thessalonika Galerius's, the mausolea at St. Peter's in Rome were both imperial, and Hagia Sophia was a cathedral as well as lying by Justinian's palace and thus serving as a palace church, a relationship which was mirrored at Aachen. Although San Vitale was not a mausoleum, it had very clear imperial connections in the form of Justinian and Theodora's portrayal in mosaic.

San Vitale is very often taken to have been the model for the Aachen Minster because of other connections with Ravenna. San Vitale is not the closest architectural model, but no one building could account for all the different components of the church, such as the imperial throne and the connecting porticus and certainly not the high west work. Most concede, therefore, that looking for a model is wrong. Krautheimer (1942) is clearly correct in stressing that the Carolingians did not, at the other extreme, simply borrow anything and everything 'antique' because it was considered to be superior. Although it is possible that San Vitale was thought by the Carolingians to have once been Theodoric's palace church and that what they supposed to be Theodoric's statue may well have been that of the emperor Zeno, this only reveals the Carolingians to have been fallible, not that their borrowings were indiscriminate. The context from which architecture was borrowed was very important, even if in doing so

new Christian interpretations were thrust upon pagan works.

One of the few new interpretations, or perhaps nuances placed on ecclesiastical architecture and adopted to fit royal residential palaces, was the round church, and its frequent dedication to St Mary. Otherwise there seems little to distinguish private church architecture from the mainstream of ecclesiastical, architectural developments. The study of palace chapels has been undertaken, despite the paucity of archaeological evidence, because the textual evidence is plentiful (Streich 1984). And Aachen Minster was, if you like, the palace chapel writ large (in fact a private chapel certainly existed there apart from the Minster).

Archaeological evidence exists for the palace chapels or churches at Aachen, Paderborn, Frankfurt, Karnburg, Regensburg, Ulm, Düren, Zülrich, and Zullenstein (although palace is surely too grand a word for the latter), among others. While at Nijmegen and Ingelheim the evidence for the large palace churches reveals them to be late, it is surely wrong to think of the palaces as having had no chapel in the Carolingian period. Either a nearby church served, or a smaller building that was subsequently replaced when the larger church came into service and now escapes archaeologists' recognition.

There is textual reference to palace chapels at Gondreville, Marlenheim, Pavia, Ponthion, and Soissons. At Quierzy, Herstal, and Samoussy major religious festivals were celebrated, so churches must have been present. But the most telling inadequacy of the evidence is the fact that Streich was really unable to discuss Merovingian royal chapels at all, given the lack of data. Yet the very frequency that monasteries and nunneries were founded in palace complexes, and even came to supersede the palace entirely, is sufficient proof of conventional piety that the absence of *oratoria*, *capellae*, or *ecclesiae* is unthinkable. It may be a reflection on how inconsequential the five villas of the *Brevium Exempla* are, or that a royal visit was never a serious possibility, that only one had a chapel. But it would appear that most royal residential villas were furnished with a church or chapel.

How common such churches were on nobles' residential villas we can only begin to guess. We know that bishops and abbots frequently built churches on their property. Theodulf, bishop of Orléans and abbot of Fleury, built a marvellous church at his villa, Germigny-des-Prés, in imitation of the Minster at Aachen, according to documentary evidence. Einhard had churches built at Steinbach and Seligenstadt, property that had earlier belonged to Count Drogo. At Steinbach an earlier wooden church (*basilica lignea modica constructa*) was certainly Drogo's. We find Bishop Bernold consecrating a church on the *villa Miliacus* and another at *villa Soloniacus* in the mid-ninth century (Imbard de la

Tour 1899, 112-3).

Some idea of the percentage of villas with churches can be gained from some monastic records of their estates. While Brioude's cartulary revealed only 32 churches or chapels on 200 villas owned, the Massif Central had the lowest figures recorded by Imbart de la Tour. There one often finds a church on one of the monastery's villas, as at *Curtis Anglaris*, and a number of other estates making up its *parochia* (in this case at least four other villas). By contrast, in the north eight out of ten villas owned by Saint-Bertin monastery had villas; thirteen of fourteen villas owned by Saint-Sulpice monastery in the deep south did too.

Was this church-building practice as common among lay nobles? The very fact that both Church and state combined in the ninth century to try to prevent lay lords from subdividing their church's *parochia* by founding a new parish – often when splitting inheritance – reveals how common villa churches were.

The probability of chapels being nearly ubiquitous becomes more apparent when one notices how frequently churches are found on sites that can be identified in textual sources as *castra*, *oppida*, *curtes*, or *loci*, not to mention the numerous small sites that have not had their designation preserved for us in writing.

Archaeological evidence of churches comes from a number of ducal residences, although some of these could easily be ascribed to royalty. Particularly well-represented are Bavarian palaces of the Agilolfings; at three of these the churches have been excavated: Regensburg, Salzburg, and Freising. A church was certainly present at Altötting. Duke Eticho allowed his daughter to found a nunnery at Odilienberg (fig. 4.25). Duke Heden had missionary churches built on his properties at Würzburg and Hammelburg. Duke Liudolf had a religious house founded at his Gandersheim villa, while Count Waltbert transferred relics to his villa, and one supposes church, at Wildesheim.

Archaeological evidence of churches also comes from a variety of important excavated sites that I interpret as nobles' residences rather than 'imperial forts': Erfurt, Würzburg, Stöckenburg, Büraburg, Amöneburg, Christenberg, Heinebach, Vogelsburg, Höfe bei Dreihäusen, Linz, Hohensyburg, and Herstelle (see Streich 1984).

Other Buildings. An unedited manuscript from Laon records what one Carolingian writer believed was essential for a palace to be worthy of its name: reception rooms, dining rooms for both winter and summer, baths, a 'gymnasium equipped for the practice of various arts', chapel, lodgings for clerics, apartments for guests, rooms for administration, a safe-room for the royal treasury, and

quarters for armed men (Martinet 1966). This was no more than the essential apartments that would be seen by the visiting elite. Behind the scenes there would necessarily be much more.

The *de Villis* capitulary makes reference to the working rooms for women, whose quarters are to have 'strong doors' and be enclosed by 'good fences'. There are wine-presses, store-rooms, bakeries, kitchens, stables, mills, and the whole range of agricultural estate buildings: byres, barns, pig sties, sheep-folds, and goat-pens. In a poem on sobriety, Milo of Saint-Amand described the palace kitchens as 'smoking day and night, and the cooks all sooty and blackened by the smoke' (Riché 1978, 96).

The *Brevium Exempla* lists Asnapius as comprising, in addition to the chambered, eleven-roomed, stone-built royal *domus*: 17 other wooden houses, 1 stable, 1 kitchen, 1 bakery, 2 barns, and 2 haylofts.

Of these quarters and ancillary buildings we know even less than of the residential *domus*. But their existence is certainly attested archaeologically. At Hünenkeller the remains of five buildings were uncovered, at Höfe bei Dreihäusen there were four in addition to a church. Typically, no function can be assigned to these structures. Indeed, I am not happy about calling the most sturdy building that happened to be uncovered at Höfe the main hall; the *domus*, I believe, remains to be found.

While there is too little space here to do more than mention these other buildings, two observations are worth making concerning their composition as a whole. The first is that there is little order. Buildings are jumbled about within enclosures without much rhyme or reason. The only exception to this is the tendency for buildings to back on to the enclosure wall, sometimes as lean-tos. We cannot even count on the main hall or chapel occupying the most elevated point. The other observation, and rather more equivocal, is that the ensemble of buildings does not reflect an agricultural, utilitarian emphasis, at least not in the same way Warendorf village settlements do. This is particularly true of Christenberg and Büraburg where regimented, continuous, contiguous buildings are found in short stretches of excavated area directly behind the rampart wall. While usually interpreted as barracks for a garrison, the possibility that these were quarters for slaves or close dependants is seldom proposed.

There is also an increasing use and embellishment of towers, especially gate-houses. Such gate-houses are striking in the *Brevium Exempla*, they are prominent at Aachen and Ingelheim, and small stone-built gate-houses or wall towers with rooms are found at Samoussy, Christenberg, Büraburg, and Höfe bei Dreihäusen.

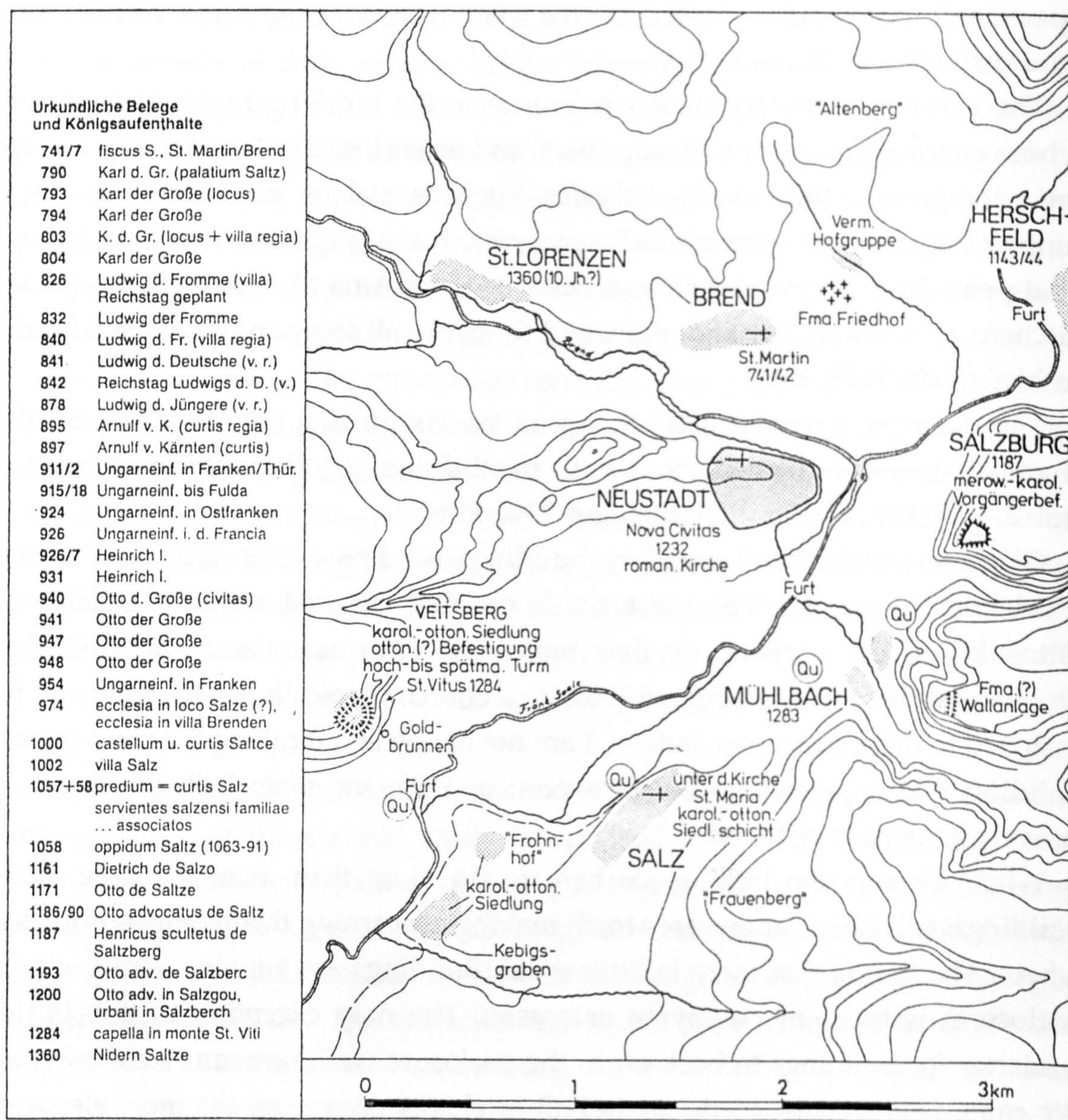


Fig. 7.14 Topography of Salz. Note that there are no less than three possible early medieval enclosures on the spurs overlooking the valley. Salz itself, and presumably by St. Maria, was the site of the important Carolingian royal palace (after Wamser).

Topography

Gauert's (1965b) summary of what is known about the structure and topography of Carolingian palaces has been widely accepted. Scholars frequently cite him when they state that Carolingian palaces were not situated in noticeably defensible positions.

Aachen lay on a slight eminence, but was dwarfed by the surrounding hills. Samoussy lay on a slight rise in such low lying grounds that it was probably marshy. Düren lay on an almost completely level, low terrace of the Ruhr.

Frankfurt, some 150 metres from the Main, sat only a half a dozen metres above it, and was lower than several other nearby rises in the land. Ingelheim overlooked the land between it and the Rhein, although the slope behind Ingelheim continued to rise another 100 metres above it. Forcheim lay in the river valley of the Regnitz, Altötting and Doué-la-Fontaine were situated, like Aachen, on a broad low eminence. Aibling and Ranshofen, on the other hand, were found in commanding positions on high terraces.

Gauert confined himself to Carolingian palaces that had been detected archaeologically as well as documentarily. Both Zürich and Bodman from the last chapter could be added to the list: situated on a small rise next to a large body of water. Zullenstein lay so close to the Rhine that ships could probably have landed less than ten metres away from the hall.

If one were to extend the investigation to include the modern sites at which we know a Carolingian villa was present, although archaeologically not yet uncovered, including Herstal, Nijmegen, and Thionville, the pattern would be reinforced. In short, one could repeat the investigation I undertook in chapter five of Merovingian royal villas with 1:25,000 maps. Indeed, in chapter five I did include two Carolingian villas that happened to fall in the same area of the Merovingian sites, Brienne-le-Château and Orville. Work on the topography of the Carolingian palace of Salz a. d. Saale shows what could be done more extensively (fig. 7.14).

Having already quoted fifth-century and Merovingian poetry depicting the topography of nobles' villas, which so closely follows the strictures set out in texts for Roman villas, I continue in the same vein here, but only give the example of Angeac and Ingelheim from Ermold the Black's *Poem to Louis* (lines 7-14, 2062-3):

Angeac

There is a river in our country of great fame,

its name is Charente and is of great honour;

Here is suited to fish and grassy meadow borders

the inhabitants of Saintes and no less of Angoulême will confirm:

Golden fields flourish and rosy meadows,

there abounds fertile fields and grapevines.

Not far in the distance you will see the nearby *laqueata palatia*,

which, Louis, carried out your wishes.

Ingelheim

This place is sited by the rapid river of the Rhine,

ornamented by various cultivated and feastful fields.

Some Concluding Remarks

Most of the conclusions I would like to draw will be dealt with in the next, concluding chapter. Here it remains only to say that the single greatest problem

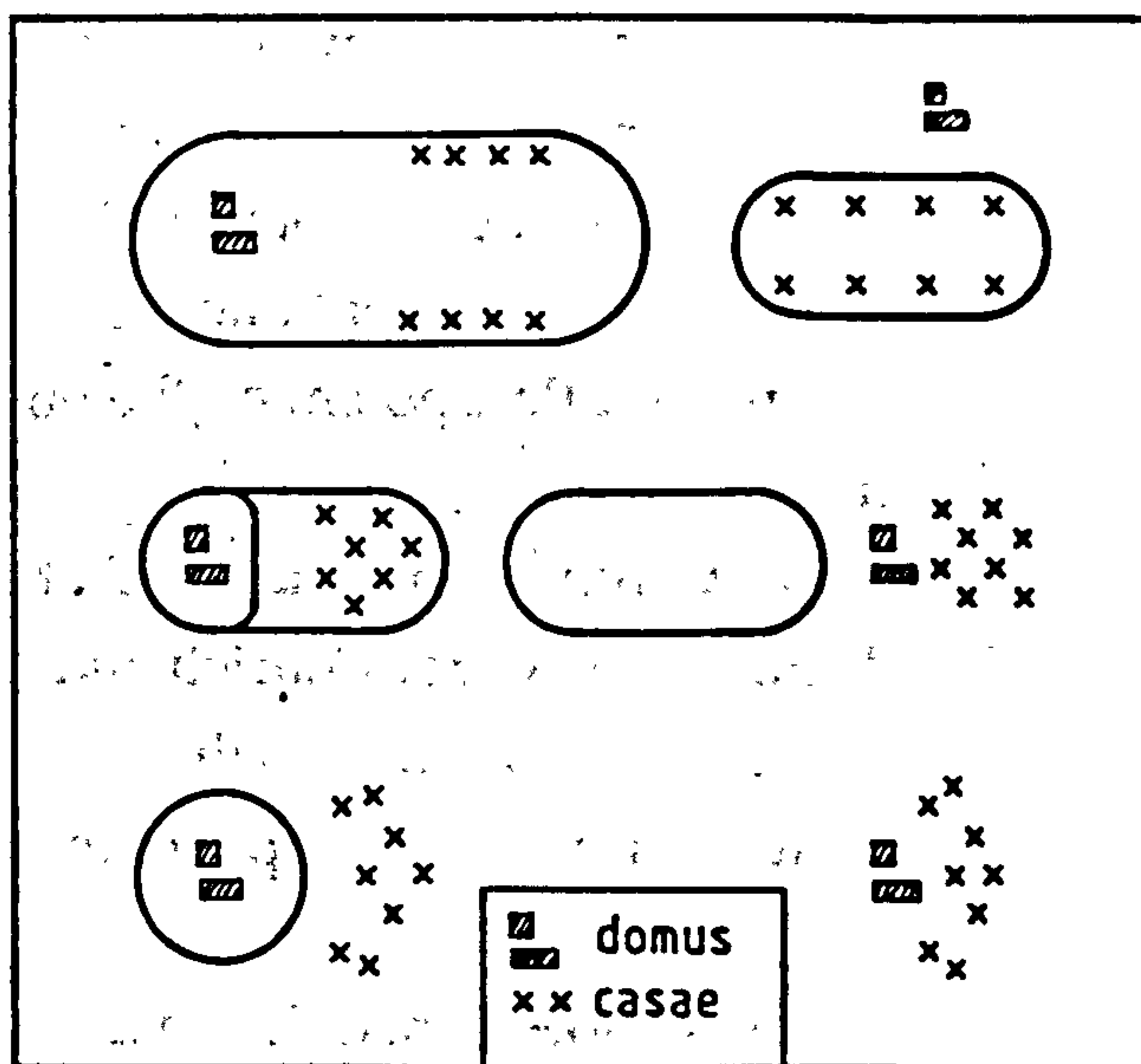


Fig. 7.15 Schematic representation of six ways a lord, dependants, and fortified enclosures could be arranged spatially, not including the important possibility of peasants being removed from the site altogether. In the bottom right case there is no fortification.

to plague the archaeological study and understanding of Carolingian villas is that in Germany, where most of the evidence has been uncovered, muddled archaeological attempts to use textual sources and royalty-obsessed constitutional historians have combined to create a myth that ascribes Frankish military garrisons to almost every enclosed site excavated. Almost nothing is left over to be ascribed to the nobility as ordinary lordly residences. The exceptions to the rule are allocated to dukes, as royal substitutes, but the same constitutional position is accepted. This tendency becomes all the more clear if one turns to work done in Bavaria. There the same ninth-century enclosed sites are to be found, but the assumed relative political autonomy of Bavaria finds its expression in the social interpretation of these sites: almost all are seen as the residences of local nobles, who exploit the peasants of the neighbouring countryside (fig. 7.15). A similar thing is found in the writings of Saxon archaeologists.

The time has come for German archaeologists to abandon their interpretations of these *Burgen* as military marching camps and garrisons and look more realistically at the society revealed by the textual evidence, a society dominated by lordship and social dependency, through which relationships part-time armies were occasionally thrown together to tramp around foreign parts for a few summer months. A society in which most of the local power and authority was exercised from the lord's *curtis*, not the king's fortress.

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Chapter Eight

Conclusions

What was left undone

Many lines of inquiry remain to be followed by future research, touched on in this thesis. For the fifth century, more work must be done to test the idea that late Roman villas formed the core of early medieval settlements, particularly the villas and *curtes* of nobles. A closer investigation of excavated Roman villas, searching for later occupation will probably not prove fruitful. Working backwards, from known or supposed early medieval villas, promises to yield considerable returns. The known or supposed early medieval villas and settlements that might be tested include sites named in early medieval texts, presumed early place-names, and sites of early churches.

At the other end of the period covered here, the Carolingian period, much more work remains to be done to glean information from written sources. I have relied largely on archaeological evidence, but the meagre Merovingian sources were shown to have much of architectural interest.

Another approach that could be applied more extensively is the use of large scale maps to study the topography of villas known from textual sources. A useful study would be one that looked at a small area for which much if not most of the settlement structure could be recreated and mapped. On a micro level, mapping of estate boundaries would be an important contribution. Whether it can be done well is another matter. Not included in the final thesis is a chapter on estate sizes and boundaries, which included a look at Corbie, Stavelot-Malmedy, Tresson, Talmas, and Tremolat. Unlike Anglo-Saxon charters, it is rare to find boundaries mentioned in Merovingian and Carolingian charters. The use of the term *finis*, apparently referring to a piece of land in charters, might provide clues to the edges of estates. But two problems dog all study on early medieval estate boundaries. The first is the long-held belief that the *communes* of France, ultimately the product of parochial divisions, fossilised villa estates. While a fine theory there is no good evidence for the belief. The second is that what we do

know about Merovingian and Carolingian villas suggests that the estates did not comprise contiguous plots of land. Instead, it seems certain that ownership of land created a patchwork. Not in the sense of common fields, but in the sense that much tenanted land could not be said to form part of the estate, so far had it slipped from immediate control; in the sense that even quite small fields, surrounded by one villa's land could have belonged to another lord, long distant. The reasons for partition, the means of acquisition conspired to keep estate composition changing. The variety of levels of dependency of peasants and their exploitation combined to make the concept of a physically unified single estate almost impossible.

So much of previous scholarship has aimed at questions of estate ownership, forms of exploitation, and social organisation. It was therefore, with no little regret, that I have avoided spending much time on these topics. But a better understanding of the physical surroundings of villas will lead one day to a better understanding of social organisation; they formed the stage for social ties and daily life.

Continuity

Under the heading of continuity a whole range of topics might be considered. Continuity of ownership, of buildings, of estate boundaries, of building techniques and architecture, and of social organisation. Some of these have not been touched at all here; some have long been the concern of historians, others of archaeologists. Some have been the concern of this thesis. No final decision can be made whether there was or there was not 'continuity' for history is, quite simply, change. The 1970s and to a lesser extent the 1980s saw a 'continuity' craze in archaeological interpretations, some of which was misguided. The question of continuity should rather be used as an analytical tool for the study of change, its pace, its direction, its causes.

Without a doubt many changes occurred following the collapse of the Roman empire in the way society was organised, in the nature of social relations and economic activity, the system of exchange, and the way domestic residences were built and arranged by the social elite.

It has been argued that continuity of site and buildings was much more common than has generally been assumed. Direct positive evidence is hard to find; there are few sites like Pfulzel where Roman villa fabric still makes up a large portion of the standing church's walls. It is possible that many of the Roman buildings that have been excavated did indeed continue in use, kept up like the Basilika in Trier or the *praetorium* in Köln, or the town walls that we read about in

Merovingian texts. However, dating of final abandonment has not been easy, finds are scarce, and the question has figured less prominently among those that archaeologists ask than has done the question of a building's first construction.

More important is the circumstantial evidence, the clues pointing towards the likelihood of continuity. Thus Roman villas are frequently found under churches and under villages, which presumably did not get built fortuitously on top of the rubble centuries after abandonment, but rather represents the outcome of continuous settlement. In addition there is the negative evidence of 'missing' Roman villas, villas that must once have existed but do not appear on modern archaeological distribution maps. These are plausibly hypothesised to lay under modern towns or villages, obliterated by centuries of building. Here again continuous settlement can be postulated.

No amount of enthusiasm for a widespread survival of buildings from the late Roman period can obscure the fact that building standards declined. Fewer buildings were constructed of stone, more of timber. But perhaps stone-built dwellings were more common than are generally thought, especially given the classical status now held by Anglo-Saxon and German timber halls in the study of early medieval archaeology, known to have been used by the highest in society as palaces, as at Yeavering. Chapters three and four suggested that stone-built villas were probably common if not the norm in post-Roman Gaul. The use of mortar in buildings appears so commonly in all the known Carolingian royal halls, but also in the enclosure walls of large important sites like Christenberg and tiny doubtless private lordly residences such as Hünenkeller, that we can probably assume it to have been the norm for noble residences when they were not built of timber. Indeed, east of the Rhein mortar was being used in the construction of elaborate graves by the seventh century. Given that mortar had become so common in the eighth and ninth century in German-speaking regions of the Carolingian empire, we are tempted to assume the same not only in Gaul at the time, but also at an earlier period.

Some unexpected Roman elements are recognisable in Carolingian royal palaces, such as contiguous building, forming ranges and wings, fronted by galleries; the *gallerie de façade* villa in France was the standard by the late empire. This is unexpected because already in the late fourth century, typified by Echternach (fig. 3.7), the processes of disintegration and separation of buildings had begun, so that Frankish villas are assumed to have comprised a number of buildings jumbled within an enclosure wall. The reason for this was in large part because Frankish building was simply less sophisticated and continuous interconnected construction was inappropriate for the mixture of stone and

timber buildings on a Frankish villa.

One visible departure from Roman villa architecture, of no doubt profound social importance, is the change in the sizes and number of rooms one found on a villa. Rooms in a Roman villa were markedly smaller than the spacious halls one finds in early medieval lordly residences. But whereas a wealthy Roman villa easily had dozens of rooms, a Frankish villa had very few. Compare one of the luxurious villas, Zeeb, in Switzerland with Ingelheim (fig. 8.1). The *Brevium Exempla* illustrates this by almost implying that a building was single roomed, for it made the distinction of well-built stone dwellings with 'two rooms', as if this was unusual or of some significance.

Preferred topography shows marked continuity from the past. Siting by major rivers, on slopes overlooking rivers or fertile basins, choice of slight eminences to ensure good drainage of ground water rather than physical dominating prominence appears to have continued unchanged from the fourth to the early ninth century. This contrasts sharply with the preference of topography shown in the positions of eleventh- and twelfth-century castles.

Gallo-Roman villas appear to have been enclosed as a rule. All the evidence points to Frankish villas having similarly been enclosed, whether by a fence, thorn bushes, an earthen, timber, or stone rampart, or a stone wall. The functions of these enclosures were varied and I have discussed them in several other places (Samson 1987; 1989; forth a.; forth b.). They legally marked private property; they demarcated different areas of jurisdiction and legally enforceable social behaviour. They also helped to signify and reinforce forms of social dependency. The more servile the dependency the greater the likelihood of dependants being constrained by a gaol-like enclosure. A schematic representation of various relationships of lordly *domus*, enclosure, and dependants' dwellings (fig. 7.15) could be used not only to characterise different periods but different forms of social dependency, from master-slave, to employer-wage labourer.

The exclusion of dependent peasants from the enclosure around the lord's domestic residence can be taken, in very rough terms, as a sign that their labour was not directly supervised and exploited. Serfs who laboured on 'their' land and paid a lord rents and renders, even labour services such as help at harvest, need not have been closely supervised or had their status constantly reinforced by daily submission; they need not have lived in a spatial configuration that expressed their dependency. It was through land-holding that their dependency was created, maintained, and judged. But many *servi* and *coloni* of the Merovingian and Carolingian period still laboured heavily under their lord's direct supervision, or that of the bailiff. Many worked three days every week on

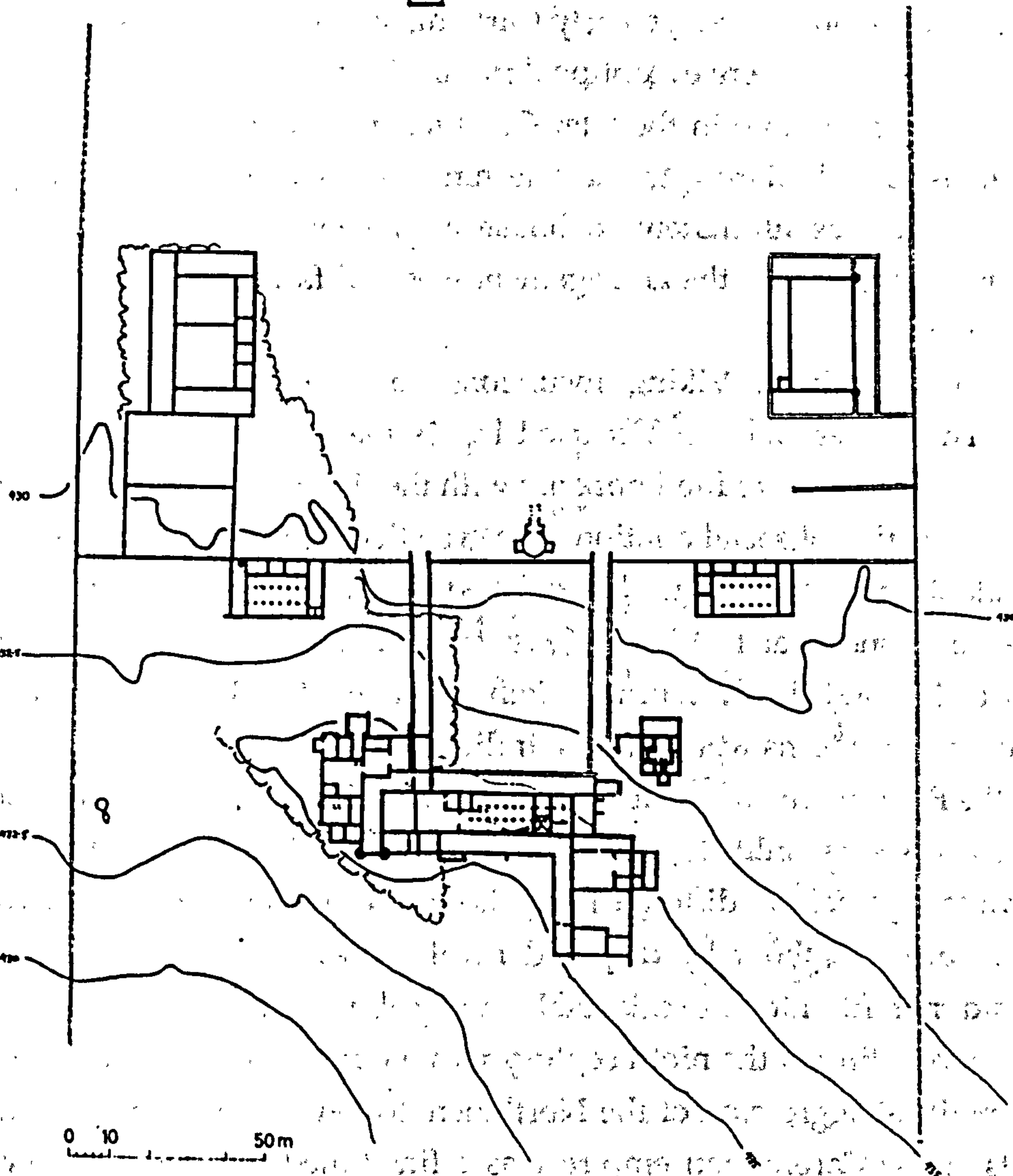
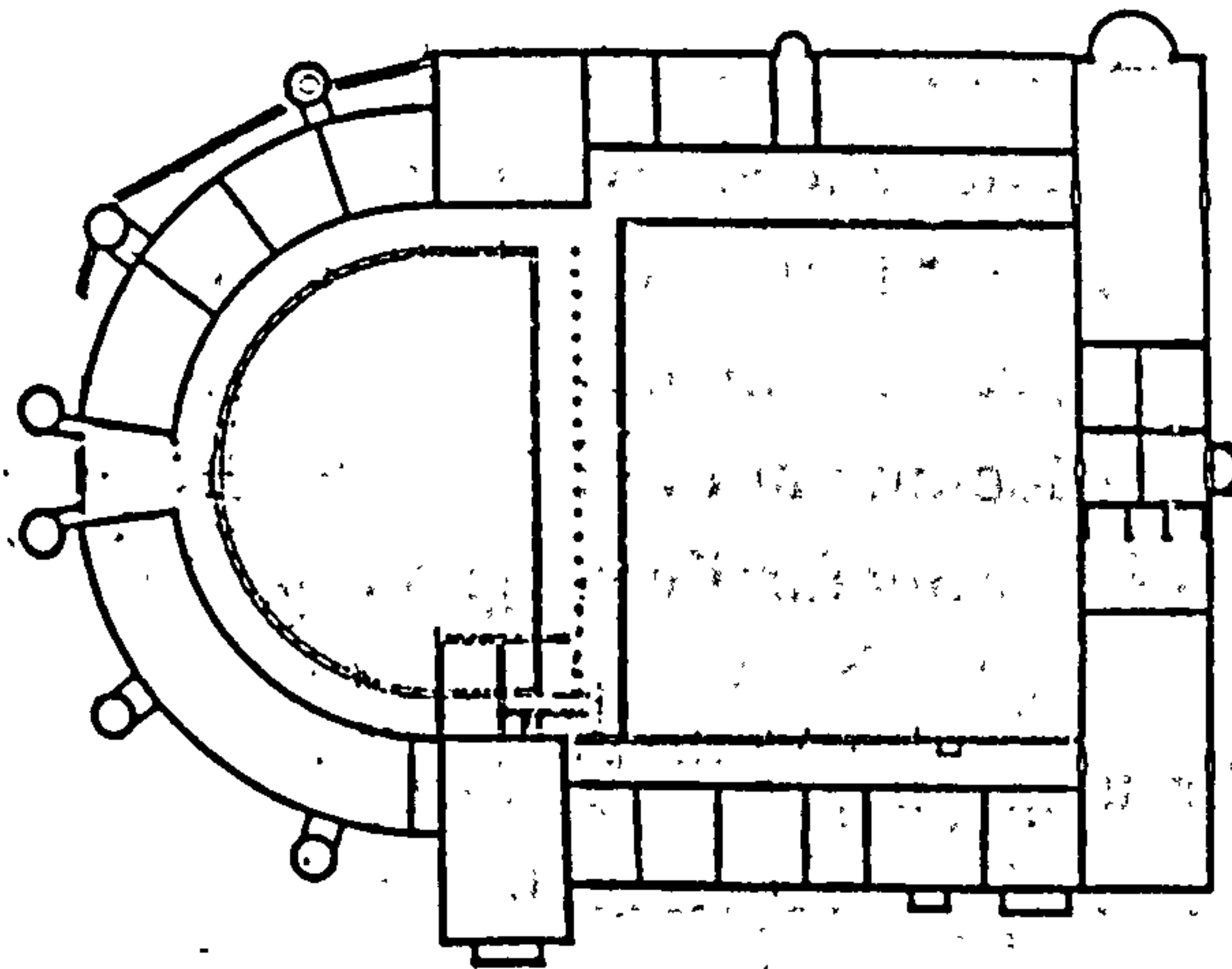


Fig. 8.1 A comparison of two large villas. One of the late Roman empire, Zeeb, the other the Carolingian palace of Ingelheim. Note how much smaller the Roman rooms are, and that the only rooms of a size comparable to the Carolingian palace were probably agricultural in nature.

the lord's fields. It is not surprising that Büraburg and even the later Tilleda had massive enclosures filled with inhabitants.

Much too little has been done to study the social implications and functions of these enclosures because few have thought of them in any terms other than defence. I have gone on at great length to try to convince the reader that defence is an inappropriate way of looking at even the very large ramparts, despite the ever-present possibility of a lordly family being burnt to death or massacred by a feuding rival.

It is no radical claim to say that villas remained relatively undefended from the late Roman period to the start of the ninth century. It is commonly accepted that the archaeologically known early Carolingian palaces were not fortified and it is unlikely that they were exceptional in this. East of the Rhein villa enclosures were often larger, tending in the later Carolingian period to enclose smaller and smaller areas. Broich, dating to the late ninth century, is a good example. The small enclosure was ultimately to house a conventional castle of the twelfth century. Elten shows that the arrangement was still fashionable in the late tenth century (fig. 8.2).

West of the Rhein, Viking incursions gave an impetus to fortification building. The process has been charted by Jäschke (1975). Traditionally Viking destruction has been accredited not only with the rise of fortification building but of the militarisation of social relations, the growth of feudalism itself (by Bloch no less). While modern scholarship has debated the size and destructive capacity of Viking armies, one clear fact has often been clouded. The Northmen caused far less destruction and death than the Goths, Romans, Franks, Saxons, Alamanni, Lombards, and Saracens were wont to inflict on each other. The wars of the sons of Louis the Pious were on a scale that dwarfed the Viking raids, even those of the great army. It seems unlikely to me that the military 'need' for defence against these invaders somehow differed from the 'need' for defence against rivals or from wars brought about by disputed royal successions. Instead, the Vikings simply did not fit into the calculable political rivalry. The sudden rush of fortification building in the ninth century was the result of an inability to reckon with the political aggression of the Northmen. Jockeying for position within the fragments of the Carolingian empire was a fine-tuned art; political power and authority was measurable in terms of allies, kin, dependants, proximity to the king, support of influential bishops, control of counts, and all the armed men this could be converted into. Political *savoir faire* helped not one jot in escaping a Viking raid.

That this is the key to the sudden rash of fortification building is visible in the

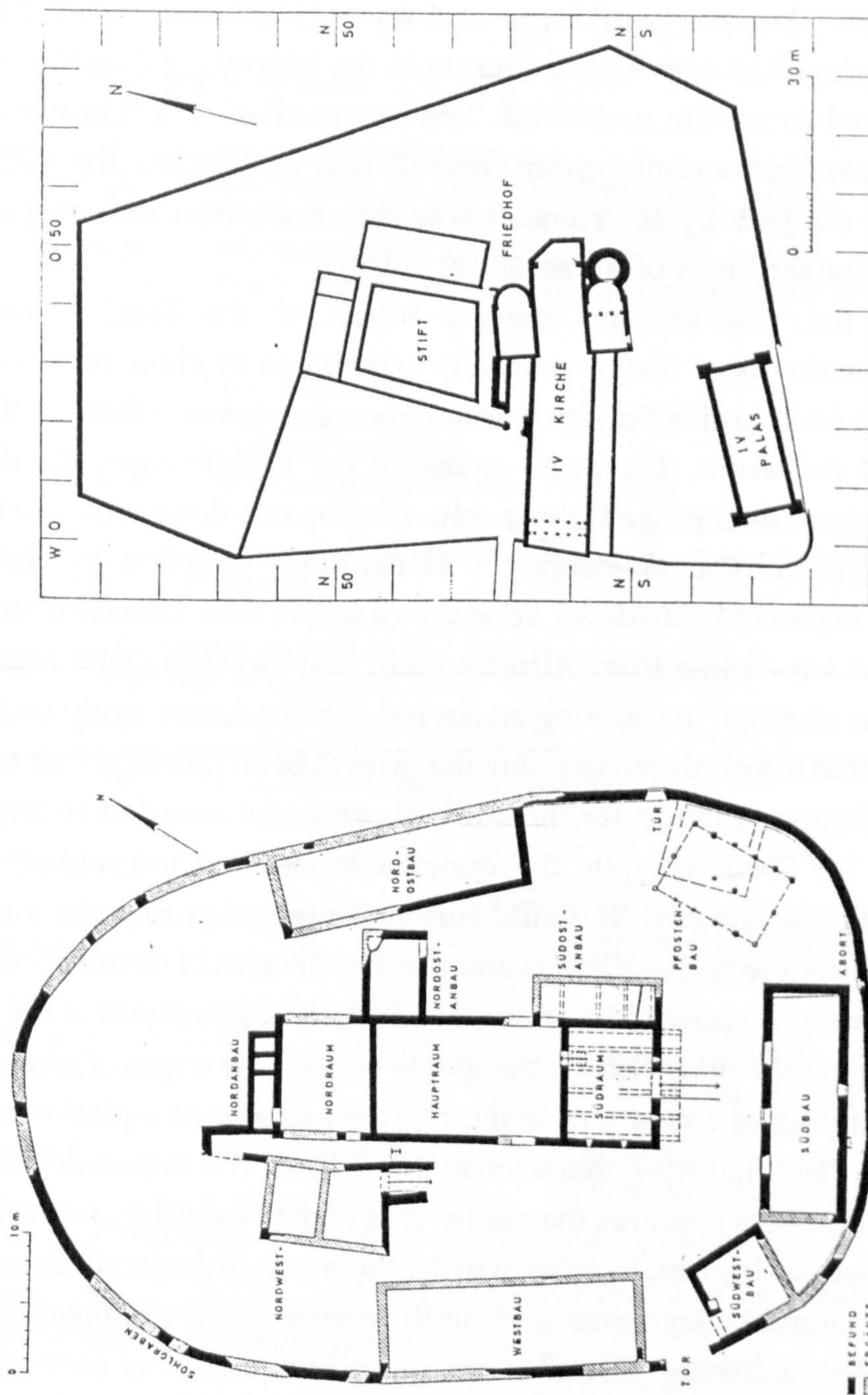


Fig. 8.2 Elten, Ottonian palace, dated to the 970s and Broich, a Carolingian villa of the late ninth century (after Binding).

actions of the Church. Unlike the barbaric Irish, Franks conducted their wars with an absolute minimum of damage to Church property. Plenty of its land was stolen, but few churches were burnt and fewer churchmen killed. The Church had always played an active partisan role in the internecine Frankish squabbles, but it expected to remain unscathed. The construction of fortifications at great monastic houses in the ninth century reveals how problematic the Vikings were, for they did not play by the rules. It was this, more than their paganism, that accounts for the shrillness of ecclesiastical outcry.

Despite the violence and martial image of the Franks, Merovingian philosophy on the social body – naturally expounded by churchmen – divided it into laity and ecclesiastics. Society was simply separated into those of the Church and those of the world. The three estates of the Middle Ages, the division of society into those who prayed, those who fought, and those who worked was a later development. The philosophy was definitely common by the eleventh century, but makes only shadowy appearance before then. One clear statement of such a world view came from Alfred's court and Georges Duby argues that it probably was common in Carolingian circles. Further, future study could test the proposition that the ninth century saw the growth of an ideology that naturalised lordly exploitation in return for the military protection afforded society by those who fought for Christ (despite the undeniable fact that the soldiers of Christ mostly fought one another). It would further be tempting to make a connection with the threat of the pagan Vikings and the first flourish of fortification building.

Here there is the possibility for some real historical analysis of the origins of castles instead of the nonsensical attempts to explain the origin of mottes as some accidental infilling of a ring fort or the repeated raising of a platform to escape inundating waters that now characterise the debate. The impression that castles were fortified houses obscures the reality that castles were, by and large, houses within fortified enclosures. In other words, there was little qualitative difference between motte-and-bailey castles and ninth-century *curtes* or *Ringwälle*. The real importance distinguishing 'castles' is not an artificial mound of earth, despite the claims of some pedantic 'castelologists'. The fundamental distinction is that some time between the late Carolingian period and the beginning of the eleventh century lordly residences had expelled almost all dependants from their enclosure: peasant dependants, that is. The enclosures became smaller, the lordly residence could be perched more precariously on top of little hills, natural or artificial, or rocky crags, while those who once lived gathered around the lord now lived apart and the early medieval villa stepped out of the Dark Ages and into the Middle Ages.

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| AA | Auctores Antiquissimi |
| BAR | British Archaeological Reports |
| CIL | Corpus Inscriptionum Latinarum |
| Fred | Chronicles of Fredegar, see Pseudo-Fredegarius. |
| GC | <i>Liber in Gloria Confessorum</i> by Gregory of Tours |
| GM | <i>Liber in Gloria Martyrum</i> by Gregory of Tours |
| HF | History of the Franks, also known as <i>Historiarum Decem Libri</i> by Gregory of Tours |
| Kr | Kreis |
| LHF | <i>Liber Historiae Francorum</i> |
| MGH | Monumenta Germaniae Historica |
| PL | <i>Patrologia latina</i> , see J. P. Migne. |
| RFA | Royal Frankish Annals (<i>annales regni Francorum</i>) |
| Settimane | Settimane di Studio del Centro Italiano di Studi sull'Alto Medioevo |
| SSR(G)(L)(M) | Scriptores rerum (Germanicorum) (Langobardicarum et Italicarum) (Merovingicarum) |
| V | Vita. |
| VK | <i>Vita Karoli</i> by Einhard. |
| VJ | <i>Vita Juliani</i> by Gregory of Tours |
| VM | <i>Vita Martini</i> by Gregory of Tours. |
| VP | <i>Liber Vitae Patrum</i> by Gregory of Tours. |
| BSAF | Bulletin de la Société Archéologique du Finistère. |

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